PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From May 1, 1879, *to December* 11, 1879.

VOL. XXIX.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
Printers in Ordinary to Her Majesty.
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ERRATUM : VOL. XXVIII, MR. GORDON'S PAPER, Pp. 156, 157.

The refractive indices given for hard crown and for light flint must be interchanged, as it has been found that the polisher to whom the prisms were entrusted has interchanged the H. C. and L. F. labels.

ERRATA IN VOL. XXIX.

Captain Creak's Paper, vol. xxix, in the Table, page 35, *for* 1875 *read* 1876.

Vol. xxix, page 140, line 12 from top, *for* $3\frac{3}{4}$ *read* $5\frac{3}{4}$.

OBITUARY NOTICES OF FELLOWS DECEASED.

The Rev. Dr. Peyton Blakiston was the youngest son of Sir Mathew Blakiston, Bart., of Sandybrooke Hall, Derbyshire. He was born in 1801, and was educated first at Eton, afterwards at Trinity College, Cambridge. From thence he migrated to Emmanuel College, where he was elected to one of the Dixie Fellowships. Having taken holy orders he became vicar of Lymington, and held that preferment for several years. His health, however, broke down, and symptoms of pulmonary disease having manifested themselves, he resorted to Paris, and placed himself under the care of the celebrated Louis, who told him that he could not expect ever to be able to discharge the duties of a public preacher. With a decision that was characteristic of him, he at once resolved to abandon the clerical profession and adopt that of medicine. For this he had always manifested a predilection, having whilst at Lymington established and devoted himself to the support of a provident dispensary.

He resigned his living, and with a wife and children returned to Cambridge, and subsequently to Paris, and devoted himself with great energy and determination to medical studies. As soon as he obtained his diploma of M.B. at Cambridge, he established himself at Birmingham, where he speedily acquired the confidence of the public and great popularity with his professional brethren. As a member of the Philosophical Institution of Birmingham, he delivered lectures on chemistry and physical science, and did good service to his profession by the application of his scientific knowledge to the elucidation of the laws of sound and the use of the stethoscope, which was then obtaining the general notice of the profession. In 1841 he took his degree of M.D. at Cambridge, and became physician to the Birmingham General Hospital. At Birmingham he published a Treatise on the Influenza of 1837, and a volume of "Practical Observations on Certain Diseases of the Chest and on the Principles of Auscultation." This volume he dedicated to his old master, Louis, and his friends and former pupils, the late Dr. W. Allen Miller and William Bowman. This volume was a valuable contribution to medical science and practice, and displayed not only a well trained philosophical mind, but also an acute careful clinical observer. In 1843 Dr. Blakiston became a Fellow of the Royal College of Physicians, and in the same year was elected a Fellow of the Royal

Society. In 1848 the state of his health compelled him to seek a more genial climate than that of Birmingham, and he removed to St. Leonards-on-Sea, where he enjoyed a large and lucrative practice till 1871, when he retired from professional labours. During his residence at St. Leonards, he published a volume entitled "Clinical Observations on Diseases of the Heart and Thoracic Aorta," and which comprised the results of his experience in a practice of more than thirty years. After retiring from practice, and while residing in London, he published a volume of lectures on "Modern Society in its Religious and Social Aspects," and a little work entitled "Clinical Reminiscences." Throughout his whole career Dr. Blakiston manifested great independence of character, a high sense of the duties and responsibilities of both professions of which he was a member, and great activity and energy in advancing their best interests. As a physician he showed much skill in the application of his acquirements as a physicist to the diagnosis of disease and evidence of carefully trained power of observation. Never seeking to win popular favour or applause for himself, he was remarkable for the interest that he manifested and the pains he took to promote the advancement of those young men of ability and promise with whom he was brought into contact. He possessed considerable general culture and artistic tastes, and his writings are scholarly, clear, and refined in style and imbued with an elevated tone. With great kindness of heart and general geniality of manner, in conversation he displayed a keen sense of humour; and, with much wit, often a degree of abruptness and sharpness of retort, sometimes a little too much for sensitive people. During the last years of his residence in London, the state of his health precluded his going much into society or taking any active share in public matters, but he felt and manifested to the last a vivid interest in all the religious, social, and scientific topics of the day. He died suddenly on the 17th December, 1878, at the age of 77.

William Froude was born in 1810, and was the son of the Venerable R. H. Froude, Archdeacon of Totnes. He was educated at Westminster and Oriel College, Oxford, where he obtained a First Class in the Mathematical Class List of 1832. Froude adopted the profession of a civil engineer, and became a pupil of Mr. H. R. Palmer, and in 1838 an assistant of Mr. Brunel, under whom he was engaged on the works of the Bristol and Exeter Railway, and on other work. For family reasons he retired from active professional work in 1846, and from that year until 1870 he occupied himself in various scientific investigations at his own expense.

Among these may be especially noted the following:—The law which governs the "Discharge of Elastic Fluids under Pressure through

Short Tubes or Orifices," on which a paper by Froude was read at the Institution of Civil Engineers, June 15th, 1847. Herein attention was called to the misconception on which was based a then generally received theory of the flow of gases. This received theory treated elastic fluids as though they were under virtually the same conditions as inelastic fluids. Froude tested by the best means in his power the result of his more correct views by experiment on the flow of air into a more or less complete vacuum.

Another subject of inquiry in which he put his views on paper (though he did not publish them) was the resistance experienced by a plane moving obliquely through a fluid. Here, as in the previous case, Froude detected and expounded quantitatively the want of correctness of the existing theory, and suggested an improved and much more correct interpretation of the experimental data. The correct theory on the subject is scarcely yet determined, though it is in course of determination by that doctrine of stream lines by the appreciation of which Froude worked so usefully.

The important bearing of the resistance experienced by an oblique plane upon the consideration of the flight of birds was a constant subject of thought to Froude. He studied the attitudes adopted by birds, especially sea fowl, in soaring, and made many measurements and quantitative calculations on the subject, but never felt satisfied that there might not be cases not to be accounted for by his interpretations.

On his voyage out to the Cape in 1878–9, and at the Cape he pursued his observations, and he wrote long letters on the subject to friends in England. One of his last acts before he was laid up in his short final illness was, though suffering from feverish cold, to measure accurately two albatross obtained for the purpose.

We will now give an outline of the investigations into the rolling and form of ships, by which Froude obtained a high position among men of science. At the request of his friend Mr. Brunel, his attention had been given to the problems connected with the rolling of ships during the construction of the "Great Eastern" ship, and in 1861 he placed before the Institute of Naval Architects a paper embodying the elementary principles by which the rolling of ships is governed.

In this paper he dealt especially with the leading phenomena of cumulative rolling, and in successive papers at the same Institution, and in *Naval Science*, he considered the modifying influence of the frictional and other resistances. He also dealt with the cases of abnormal forms of ships.

As a preliminary to the discussion of the subject of the rolling of ships, Froude had put into mechanical language the essential features of the trochoidal sea wave. He, from the first, verified the principle of the subject by experiments with variously formed models. The

consideration of the great advantages of introducing resistance as a check to rolling, led him to urge the adoption of bilge keels on an enlarged scale. The advantage of such large bilge keels was especially noticeable in the behaviour at sea of the Indian troop ships.

Contemporaneously with his investigations on rolling, Froude had been pursuing the subject of the resistance of ships, and by experiments in models tried first on a small scale and subsequently with models 6 feet and 12 feet long, had satisfied himself of the correctness of a definite law which he propounded for the relation between the resistance of a vessel and that of her model, and his law was simply that the resistance varied as the cube of the linear dimension at speeds proportional to the square root of the linear dimension. A correction had to be applied to allow for the increased ardency of surface friction on a short surface like a model as compared with that on the much larger surface of a ship.

Froude, convinced of the value of model experiments as a measure of the resistance of ships, at the desire of Mr. E. J. Reed, who had examined the experiments and their results, made a proposal to the Admiralty to conduct a series of experiments on models for them. The proposal was accepted, and in the year 1870 the experimental establishment near Froude's house at Chelston Cross, Torquay, was constructed. The main features of this are a covered waterway along which models made of hard paraffin are drawn by steam-power at regulated speed and their resistance automatically recorded. There are machines for shaping the models and appliances for testing the effect of model screw propellers and for making other observations.

At this time, also, the loss of Her Majesty's ship "Captain" led to the appointment of the Committee on Designs of Ships of War, of which Froude was a member.

As a work undertaken for this Committee, he tried a variety of experiments with models of Her Majesty's ship "Devastation" to exemplify the probable effects of bilge keels in the rolling of that ship, and shortly afterwards, at the request of the Admiralty, he determined by still water rolling experiments with several of Her Majesty's ships the resistance which the forms of their hulls offered to rolling whence might be inferred their probable behaviour at sea.

Proceeding a step further, Froude devised automatic recording apparatus for continuously registering the rolling movements of a ship at sea, both in relation to the vertical and in relation to a line perpendicular to the mean wave surface on which the ship at each moment was placed. From the record given by this instrument the form of the waves in which the ship had been resting could be deduced with accuracy.

Following up these experiments on rolling, Froude conducted experiments at sea in Her Majesty's ship "Greyhound," and sub-

sequently went on the experimental cruizes of Her Majesty's ship "Devastation," when valuable observations were made on her rolling and general behaviour at sea.

His services as a member of the "Inflexible" Committee in 1877, were the latest practical application of his studies of the rolling question.

Another outcome of the Committee on Designs was an important experiment on the resistance of a full sized ship which was conducted under Froude's superintendence.

The "Greyhound," a corvette, was towed at various speeds, and her resistance measured by a special dynamometer. The result was valuable as verifying the result of the experiments with models, and it was also valuable as calling attention to the great difference between the power delivered in a ship's engines, and the power usefully employed in propelling the ship.

The causes of this difference, especially that of the detrimental action of the screw propeller, when placed immediately behind the ship, was a matter to which Froude paid great attention, and the several ways in which the engine power is used or wasted, was the subject of one of his later papers read before the Institute of Naval Architects.

At the establishment at Torquay there have been in progress a series of experiments in the forms of ships from which valuable general results have already been obtained, and for the Royal Navy all the proportions and forms have been subjected to the investigation given by Froude's experimental apparatus at Torquay.

In 1875, as President of the Mechanical Section of the British Association, Froude, in his address explained in an intelligible form the bearing of the stream line theory on the question of the resistance of ships. This address he afterwards delivered as a lecture at the Royal Institution.

It has been said above that propulsion was a subject carefully considered by Froude. In connexion therewith, he was requested by the Admiralty to design a dynamometer, capable of testing the power delivered by large marine engines. The novel and ingenious instruments he thought out to meet the extreme conditions of transferring to a resisting lever the rotative force of 2,000 or 3,000 horse-power was described by him at the meeting of the Institution of Mechanical Engineers, at Bristol, in 1877.

He also had occasion to investigate the elementary principles determining the proportion of screw propellers; and his paper on this subject, read at the meeting of the Institute of Naval Architects in 1878, was the last of his published contributions to science.

Froude's investigations had been recognised by his election, in 1870, as a Fellow of the Royal Society, and by his receiving in 1876 the

Royal Medal from the Royal Society, and the honorary degree of LL.D. from the University of Glasgow. Although his health had suffered from domestic sorrows, it might have been expected that more years of work were in store for him, to be rewarded by new discoveries in his favourite studies. Yet these hopes were destined to be summarily disappointed. He went, in the winter of 1878, on a pleasure cruize in Her Majesty's ship "Boadicea," and while at the Cape was attacked by dysentery, and died, after a short illness, on May 4th, 1879. Froude's character was one in which a rare degree of modesty and disregard of self was combined with a singular charm of voice and manner. This enabled him so to act in many difficult positions that he secured for himself a prompt recognition of the great scientific knowledge and powers of mind with which he was endowed. Those who would desire to learn something of his character and work, as depicted by himself, would do well to read the evidence given by him before the Royal Commission on Scientific Research, on May 29th, 1872.

Baron John Benjamin Heath, Grand Officer of the Order of the Crown of Italy, Knight Commander of the Italian Order of Saints Maurice and Lazarus, Knight Commander of the Russian Order of St. Anne, Consul-General for the Kingdom of Italy, was born at Genoa on the 6th June, 1790; his father, Mr. John Heath, having been many years established as an English merchant in that Republic. A few months after his return to England, in 1798, Mr. John Heath sent his son, then only eight years of age, to school at Harrow, under the care of Dr. Drury, then head master of that school, whose wife was sister to Mr. John Heath. He passed about eight years at Harrow, where he was for some time fag to Lord Byron. On leaving Harrow he was sent to Hall Place School, near Foots Cray, in Kent, to learn arithmetic, English grammar, and other matters which at that time were much neglected at our great public schools. He married, in 1811, Sophia, daughter of Robert Bland, M.D., of London, by whom he had a numerous family, of whom seven, *i.e.*, four sons and three daughters, survive him.

In November, 1816, he was appointed Consul-General for the Kingdom of Sardinia, and in 1861 became Consul-General for the Kingdom of United Italy; so that at the time of his death, in January last, he had held his Consular Commission rather more than sixty-two years. After fifty years of Consular service he was created a Baron of the Kingdom of Italy by King Victor Emmanuel II, and the title was made hereditary through the male branch.

Baron Heath continued the business of his father, as a merchant and foreign banker, in London, and was still at the head of the house of Heath and Co. at the time of his death. He was for fifty years a

director of the Bank of England, and was Deputy Governor of the Bank, with Mr. William Cotton as Treasurer, in 1844, when Sir Robert Peel passed the new Bank Act; the two Governors of the Bank materially assisted Sir Robert in preparing the details of that most useful measure.

Baron Heath was for many years a member of the Court of Assistants of the Grocers' Company, whereof he was Master in 1829, when he presented the Company with his unpublished book, called "Some account of the Grocers' Company," which is a history of the Company from its institution more than five hundred years ago. As a testimony of their appreciation of this work, the Court presented him with a splendid piece of silver plate.

Baron Heath was elected a Fellow of the Royal Society in 1843; he was an accomplished musician and a good linguist, and took much interest in literary and archæological pursuits, in following up which, he became a member of several learned societies; he was also a member of the Roxburghe Club, of the Philobiblon Society, and the Dilettante Society; he possessed a well-selected library and a large collection of autographs, in which he took great pleasure.

Baron Heath died on Thursday, the 16th January last, after a few days' illness.

Professor Kelland was the son of the Rev. Philip Kelland, who at the time of the birth of his son was rector of the parish of Dunster, in Somersetshire. Afterwards it would appear that he removed to Landcross, in Devonshire. Though an Oxford man himself, his father sent his son Philip to Queen's College, Cambridge, where he greatly distinguished himself among his contemporaries, and in 1834 stood at the head of the honour list as Senior Wrangler and Smith Prizeman. Among those whose names appear on the same list were the Rev. Dr. John William Donaldson, the author of the New Cratylus, and editor of "Pindar;" and Mr. Main, who became first assistant in the Royal Observatory, Greenwich, and subsequently Director of the Radcliffe Observatory, Oxford. The latter subsequently married Mr. Kelland's sister, and between the two old college friends there existed the closest intimacy until Mr. Main's death. Mr. Kelland, who had taken orders in the Church of England, became a tutor in Queen's College, and continued such for the next three years. It was in 1838 that he was appointed to the Chair of Mathematics in the Edinburgh University, as successor to Professor Wallace. For the chair there were a number of candidates, including Mr. Gregory, the author of "Gregory's Examples," and Mr. Edward Sang, and a warm controversy seems to have been carried on as to the respective merits of the rival candidates. Against Mr. Kelland, Sir William Hamilton, then the occupant of the Logic Chair, was in arms, and in June, 1838,

the great metaphysician and philosopher wrote a pamphlet addressed to the Lord Provost and Town Council, in whose hands was the patronage of the chair, "on the election of a Professor of Mathematics," in which he spoke very strongly in favour of the qualifications of Mr. Gregory as against those of Mr. Kelland. Once in the Senatus, however, Mr. Kelland's gentlemanly manner, not less than his eminent ability as a teacher, soon converted former opponents into friends. That he bore no malice—as indeed his nature was incapable of doing so—to those who had opposed him is shown from the way in which he speaks of Sir William Hamilton in connexion with later fights, both in the Senatus and between that body and the Town Council. "I can say" (he remarks), "who was witness and part-actor in all these contests, that never did Sir William exhibit a shadow of self-seeking. Not for his comfort or his gain he contended; and although my own department was the object of some of his best known attacks, I can say that never for one moment did I feel towards him other than the warmest personal regard—nay, more, attachment. He was indeed one of my kindest, steadiest friends." On the retirement of Sir William Hamilton as secretary of the Senatus, another controversy arose. The Town Council proposed that the office should be merged in that of a general secretary to the University; but the Senatus carried the day, and appointed Mr. Kelland their secretary—a position he filled in a highly satisfactory manner until 1867. Official duty as well as inclination, therefore, led him to take an active part in the movement then going on in favour of University reform, which resulted in the appointment of the Commission of 1858, and the ultimate release of the University from what the Professor described as the "somewhat imaginary despotism of the Town Council." When Professor James D. Forbes, who occupied the Natural Philosophy Chair, was laid aside from work through ill-health, Mr. Kelland came forward, and, with the assistance of one of his students, discharged the duties of the chair from 1852 until 1856. As a teacher he was exceedingly popular with his students, and that the Mathematical class did not fall off in his hands he proved with justifiable pride in the speech already referred to, by an allusion to the fact that whereas in 1838 he enrolled 111 students, in the past year he enrolled 373.

The year after he came to Edinburgh, Professor Kelland was elected a Fellow of the Royal Society of Edinburgh, in whose welfare he ever took the greatest interest, and to whose publications he largely contributed. He became in time one of the vice-presidents, and last year he was chosen to succeed Sir William Thomson in the presidential chair, and opened the session in November last with the customary inaugural address. The deceased Professor was also, for about ten years from 1850, an active member of the Society of Arts, and he was president of that Society also in the session 1853–4. An institution

in which Professor Kelland took much interest was the Life Association of Scotland, of which he was one of the original founders. In connexion with the business of that institution he was induced in 1858 to take a trip to America, and while there he improved the occasion by making himself better acquainted with Transatlantic science and the state of education in the primary and secondary schools of the United States. A few of the results of his observations he gave to his friends in the shape of a small volume entitled "Transatlantic Sketches," which is written in a light conversational style. As stated above, Professor Kelland was a clergyman of the Church of England, and he occasionally officiated in St. James' and other Episcopal Churches. Preaching, however, was, in the opinion of his friends, one of the few accomplishments in which he did not excel. Professor Kelland was twice married—first to Miss Pilkington, of Dublin, and subsequently to Miss Boswell, the only daughter of the late Captain Boswell, R.N., of Wardie. His widow, three sons, and two daughters survive him. In politics he took little interest; and it is said that the only occasion on which he was known to vote was at the first School Board election, when he voted for the lady candidates.

The class-room was undoubtedly Professor Kelland's proper sphere. As a teacher he has been equalled by few and surpassed by none of the many colleagues alongside of whom he has laboured. In the Royal Society he was always regarded as an authority on mathematical and physical subjects. His notes, though not numerous, were much valued, and his criticisms were listened to with respect. But he had little ambition to shine as an explorer, whatever his capacity might be. Fate had made him a teacher, and to that work he bent all his energies. His function was less to make discoveries than to methodise, adapt, and disseminate the discoveries of others. This implied that he should be a student as well as a teacher, and one chief cause of his success in the latter capacity was his preseverance in the former. None who witnessed it can have forgotten the enthusiasm and delight with which he first assimilated and then reproduced in his class the doctrine of Quaternions developed by Sir William Rowan Hamilton upwards of twenty years ago. Then he was both student and teacher at the same moment. The members of his class were his fellow-students. His morning demonstrations were instinct with the freshness of the evening's discoveries, and his face beamed with delight and his eye twinkled with triumph as his rapid fingers worked out the beautiful results on the board. As a teacher his one fault was that he rushed forward somewhat too rapidly for the majority of his hearers. One reason of this was, that he assumed rather too high a standard of attainment on the part of entrants to the University. Another reason of it was that he had a large amount of work to do and little time in which to do it. He therefore carried on the few rather than the

many. But those who had the needful ability, and had the power of work in them, followed him with the utmost interest, and generally pronounced him, when in his prime at least, the best professor in the Faculty of Arts. Personally he was much beloved by his students. He was a man of genial temperament and kindliness of heart; and he was ever ready to help deserving students, and to encourage those whom difficulties might have deterred from presevering in their work. When acting as substitute for Professor Forbes in the Chair of Natural Philosophy, he discovered a power of popular lecturing of which he did not seem to have been himself aware, and which was quite new to his friends and even to his students. The lectures he frequently delivered before the Philosophical Institution were one result of this discovery. Of all the departments of physics with which he dealt, Acoustics was the one in which he took the greatest delight, probably because his skill as a violinist went hand-in-hand with his ability as a mathematician. Perhaps the most successful of his lectures were those on the lives and labours of eminent natural philosophers, as Kepler, Galileo, and Newton. The literary finish, the sparkle of wit, and the soundness of judgment by which these lectures were characterised make it a cause of regret that his efforts in literature were generally so strictly professional. Of his contributions to the Proceedings of the Royal Societies of London and Edinburgh and to the Philosophical Society of Cambridge, twenty-six papers are particularly specified in the catalogue of the first-mentioned scientific body. He published works on "The Theory of Heat," in 1837 and 1842; treatises on "Algebra," in 1839 and 1860; and in 1873, an "Introduction to Quaternions," conjointly with his colleague, Professor Guthrie Tait. The introductory and valedictory addresses which he occasionally delivered to his classes, sometimes dealt with the question of University reform. On this subject his views inclined to Conservatism. Two of these lectures were published—the one in 1854, entitled "The Scottish Universities suited to the Scottish People," and the other in 1855, with the title, "How to improve the Scottish Universities." Almost his latest work, and that which is most worthy of his reputation as a mathematician, is the article on "Algebra," contributed to the ninth edition of the "Encyclopædia Britannica.' He was elected a Fellow of the Royal Society in 1838.

Sir Thomas Larcom, who died at Heathfield, Fareham, Hants, on June 15, was the last survivor of that remarkable band of officers of the Royal Engineers, many of whom were Fellows of this Society, whose names will never be forgotten in connexion with the Ordnance Survey of Ireland. That Survey, the model in its grand comprehen-

siveness of the Survey of Great Britain, is perhaps, considering the period at which it was executed, the highest result of British practical science of which this country can boast, and was the work of no common men. As they have been removed one by one from among us, notices of Colby, Drummond, and Portlock have appeared in our pages; Murphy was drowned in the Euphrates on Colonel Chesney's Expedition; Robe, Sir William Reid, and Dawson passed away long ago. Of these General Colby was the master and tutor. Selected in 1824 by the discerning eye of the Duke of Wellington for the task of executing the Survey of Ireland, and leaving it completed in 1846, his success was what might have been expected from the well-proved fertility of his inventive genius, his extraordinary force of character and self-devotion, the high moral tone which was sure to propagate itself amongst his staff. If others invented, enlarged, improved, suggested, executed,—to Colby, the Commander-in-Chief, the main credit of the Survey must always belong.

The only officer of this band to whose career, as a whole, that of Larcom bears any similarity, is the lamented Drummond; but Drummond's early death prevented the full development of talents which would certainly have raised his already great fame to a still higher level than it actually attained; Larcom was permitted for a period of more than forty years to become identified with what may be called his adopted country; and his memoirs would be to a very great extent a history, for his period, of Ireland itself. Like Drummond, the work which he accomplished on the Irish Survey pointed him out as the proper person to organize and administer various branches of the Civil Service; and like him, he was made Under-Secretary for Ireland, with the marked approval of the public. It is of this varied service that we must make a brief sketch.

Sir Thomas Larcom resembled Generals Colby and Portlock in one respect, rather than Drummond. Like the two Generals, he was the son of a distinguished sea officer; and the sense of duty, the undaunted energy, the practical sagacity which all three inherited, may very clearly and very similarly be traced in their career. His father, Captain Joseph Larcom, R.N., who saw much active service, was best known by the office he held for some years as Commissioner of Malta Dockyard, on his way home from which post he died in 1817. Sir Thomas was born in 1801, joined the corps of Royal Engineers at Chatham, with a high reputation from Woolwich, in 1821, and served his first two years at Gibraltar. His early character, on its scientific side, was formed under men of genius, General Mudge, R.A., the successor of General Roy in the first operations of the English Survey, and Sir Charles Pasley, the founder of the Engineering School at Chatham. Under these officers, while learning the rudiments of his profession, he imbibed his full share of that combined military and

scientific zeal which grew out of the experiences of the Peninsular War, and was perhaps best represented before the eyes of later generations by Sir John Burgoyne, one of Larcom's truest friends. In 1824, Larcom commenced his labours in the Ordnance Survey of England, but was selected by Major Colby for the Irish Survey in 1826. Here he found himself happily coupled with one who had been known to him from boyhood, the late General Portlock, in those arduous operations of the Trigonometrical Survey by which Ireland was connected with the sister island in a network of triangulation. In 1828 he was entrusted by General Colby with the central organization at Dublin of the whole Survey. To him was assigned, under the immediate command of his chief, the task of examining and comparing all the plans and documents as they were sent in from all portions of the Survey, of compiling them into county maps, engraving, and finally publishing them. To him fell the training and discipline of an army of draftsmen, computers, engravers, and printers, a large proportion of whom were men of his own corps. While in this position, he deserves the credit of adopting and of adapting every invention, such as that of electrotype, by which economy could be secured and production multiplied; and every Continental improvement, such as that of Contouring, a novelty at first warmly opposed, but gradually triumphant. In the course of a very few years the establishment at Mountjoy, in Phœnix Park, was without an equal in the world, the pride of Ireland, the admiration of scientific travellers. It was afterwards transferred to Southampton under the able superintendence of his brother officer, Sir Henry James.

But Larcom was not only distinguished as a scientific man, he was pre-eminently the literary man of the party; and his mental activity was early displayed in promoting that enlargement of the original idea of the Survey which General Colby was wise enough to encourage. In the preface to the "Memoir of Templemore," the General says:— "Lieutenant Larcom conceived the idea that with such opportunities a small additional cost would enable him, without retarding the execution of the maps, to draw together a work embracing every description of local information relating to Ireland. He submitted this idea to me, and I obtained the sanction of the Irish Government for carrying it into effect. To him I have entrusted the execution, and this volume is the first public result." Colby himself, as his biographer, General Portlock, admits, "was not an antiquarian;" Larcom was. He applied himself to the study of the Irish language, in order to qualify himself for his self-imposed task; and under his enthusiastic guidance competent persons searched every record, and made every personal investigation necessary for the historical and literary branch of the Survey. A vast body of information, of the highest value, was gradually collected; much of it already digested;

the ideal would shortly have been realized, but, from motives of economy, the Government suspended operations, and the organic whole, to Larcom's intense disappointment, was not to be the crowning glory of the Ordnance Survey. Its component parts, however, were in time undertaken by various scientific bodies, some of which took their rise from these very operations now suspended. Years afterwards, the President of the Royal Irish Academy, the present distinguished Bishop of Limerick, referred to Sir Thomas Larcom in the following words:—"For my own part, I do not hesitate to express my belief that to the individual who planned and organized it [the Survey, as to its literary, historical, and statistical branches] we are indebted for some of the most important advances which have been made within the last quarter of a century in the studies relating to the history, the languages, and the antiquities of Ireland. To Lieutenant, now Major-General Sir Thomas Larcom, belongs this honour." ("Proceedings of the Royal Irish Academy," VII, 437.) During the eight concluding years of the Irish Survey, Larcom had the entire executive charge of it, his chief's presence being required in London for the English Survey.

In 1846, when the Irish Survey was completed, Larcom was finally adopted into the Civil Service of Ireland, as a Commissioner of the Board of Works; but he had already been an Assistant Commissioner in special connexion with the works systematized by way of relief to the sufferers by the Irish famine. The immense labours of that and the next year, which were required of him as the chief director of these works, nearly brought him to the grave; and indeed it would have been impossible that he could have carried them through, had not his previous employments given him a minute acquaintance with every necessary detail. In 1832 he had prepared for Government the plans required for the Irish Reform Bill; in 1836 he had prepared the topographical portion of the Report on Irish Municipal Reform, when elaborate plans and maps of sixty-seven towns were prepared and presented in the space of one month; in 1837 and 1838 he had executed, with a view to commencing the Irish Railways on a scientific plan, the first general map ever made in Europe on which the ground was completely delineated as one whole. In 1841 he had been a Census Commissioner, and made the great statistical advance of including in the Report an account of the classification and condition of the population as well as its numbers, an extension of the old method since adopted in Great Britain and on the Continent. In 1842 he had been a Commissioner to inquire into the state of the Royal Irish Society, and in 1845, for purposes relating to the new Queen's Colleges.

For none of these Commissions or other work of the sort would he ever receive any remuneration; and to this resolution he adhered,

when, in subsequent years, he collected Returns of agricultural produce for the Government, when he was made Chief Commissioner for inquiry into the Irish Poor Law in 1848, and in the next year held the same place in the Commission for the reform of the Dublin Corporation. The importance of the Reports of the Boundary Commissions, over which he presided, may be judged by the fact that the Unions and electoral districts of Ireland were remodelled in accordance with the suggestions contained in them. In 1850 he became Deputy Chairman of the Board of Works; and in the year following, a member of the Senate of the Queen's University.

It will thus be seen that in the process which placed Ireland, during a rapid course of scientific inquiry and judicious improvement, far ahead of the larger island, Larcom was certainly one of the principal agents; and, indeed, it would be difficult to mention any one name more immediately concerned in producing that result. It was almost, then, a matter of course, that when a vacancy occurred in the Irish Under-Secretaryship, he should be selected for that post in 1853; but it distinguishes him from all others who have held it, that the office was for the first time made non-political and permanent, in order to keep him at the helm; while since his retirement, after a service of nearly seventeen years, its permanent character has been, perhaps unfortunately, abolished. During the Viceroyalty of Lords St. Germans, Carlisle, Eglinton, Kimberley, and the Duke of Abercorn, the subject of this memoir passed through the several gradations of military rank till he became Lieutenant-General, and was decorated with the K.C.B. It was during the latter part of this time that the Fenian Insurrection threatened, developed itself, and—greatly owing to General Larcom's incessant vigilance and consummate precautions—was quelled. The interruption it caused to the steady flow of Irish prosperity, which had set in after the famine and subsequent emigration, was a serious grief to the Secretary who had done so much in the previous years to remove the incumbrances of ages, and to foster the material prosperity, the education, and the social improvement of the Irish people. To name in this sketch the numerous instances of the sort, which are indeed well known, would be quite beyond our limits. For some time before his retirement he had begged to be relieved from an office which had begun to overtax his energies; but he could not be spared; nor was he the man to desert his duty as long as it was believed by his superiors that his services were necessary. When he retired, in 1869, the Government of the day bestowed on him the Baronetcy he had so well deserved, the Irish Privy Councillorship, and a pension equivalent to his pay. The exhaustive minutes drawn up by him for successive Governments, many of which furnished the speeches of Ministers and Viceroys, form of themselves a history of the progress of Ireland; but, perhaps the best tribute to

his unwearied diligence and great powers of administration, is the saying which grew to be proverbial, not only in Ireland but beyond it, that whatever the changes of Lord-Lieutenants and Chief Secretaries —and he served with some excellent ones—Ireland was always really governed by "Larcom and the Police." It may be questioned whether it was ever better governed. The meaning of the expression was this:—Ireland had a long breathing-time from the troubles which had so often marred her prosperity; religious and political differences were restrained by the strong hand from disturbing the peace; and the grievances which were inseparable from such a past history were mitigated by the general sense entertained of the perfect fair dealing which prevailed at head-quarters.

This, at any rate, was the interpretation put upon his services by those best able to judge. On his resignation, Addresses, accompanying splendid gifts in plate, were presented to him by the leading noblemen, gentry, magistrates, and scientific men of Ireland; and the following sentences may be quoted:—

"Under six successive administrations, you have discharged the duties of your office with credit to yourself and with advantage to the country, with exemplary diligence and fidelity, for the preservation of social order, and the promotion of national prosperity.

"We also desire to express our grateful appreciation of the earnest and efficient manner in which for a period of forty years and upwards you have laboured in different departments of the public service to make known and turn to good account the national resources, to advance science and literature, and collect materials for the elucidation of the ancient history of Ireland."

It will be gathered from the sketch of Sir Thomas Larcom's life here presented, that he was too busily occupied in administrative labours to leave behind him any extensive contributions to literature. He was rather the instigator and pioneer of scientific research, the founder or co-founder of scientific Societies, the dispenser of assistance to them from his position in the Government. But, besides the volumes of the "Irish Ordnance Survey," which have been mentioned as chiefly compiled by him, his edition of Sir William Petty's famous "Down Survey," is a work of merit; and he contributed several articles to the "Aide Mémoire," as well as very interesting and better-known memoirs of his friends Drummond and Portlock to the "Professional Papers of the Royal Engineers."

Sir John George Shaw-Lefevre, K.C.B., was the second son of Charles Shaw-Lefevre, Esq., of Heckfield Place, Hants, for many years Member for Reading; the eldest son, at an early age, became Member for the County of Hants, and filled the office of Speaker of

the House of Commons from 1839 to 1857, when, on his retirement, he was raised to the Peerage under the title of Viscount Eversley.

The second son, John, was born in 1797, and was educated at Eton and Cambridge. At the University his career was most honourable; though of feeble health, which at that time never permitted him to work for more than six hours a day, he was Senior Wrangler in 1818, a year remarkable for the number of men of ability who passed in honours and who subsequently distinguished themselves; he was elected Fellow of Trinity in the following year, and was called to the Bar by the Inner Temple in 1824. For some years he practised as a conveyancer with success; but after a time was gradually drawn away into political employment. In 1832 he was employed by the Government of the day to arrange the divisions of counties for representation under the Reform Act. In the same year he was elected member for Petersfield by a majority of one, but was unseated on a scrutiny. Shortly after he was appointed Under Secretary of State for the Colonies, by the Secretary of State, Mr. Stanley, afterwards Earl of Derby. In 1834 he undertook the work of Poor Law Commissioner, under the Act passed in that session for the amendment of the Poor Law. His work involved the entire organisation of the new system, the arrangement of Poor Law Unions over the whole country, and the building of workhouses, in the face of vehement opposition from a large part of the Press and the public. He held this post for seven years, till 1841, when the work began to tell upon his health, he then exchanged this office for that of Secretary of the Board of Trade, which he held till the year 1848, when he was appointed Clerk of the Parliaments, an office which he held till his final retirement in 1875, four years before his death. His official duties connected with these offices formed, however, but a small part of his work; he was employed by successive Governments in a great number of Commissions and Inquiries involving great labour. In 1834 he was one of the Commissioners entrusted by Lord Glenelg with the foundation of the Colony of South Australia; he was also one of the Founders of the University of London, and in 1842 succeeded the late Sir John Lubbock as Vice-Chancellor of the University, a post which he filled for twenty years till 1862. In 1842 he was a member of a Commission to inquire into the recent losses of Exchequer Bills; in 1843 he was appointed member of the Emigration Commission; in 1846 he was employed by the Government on a mission to Edinburgh, which resulted in founding the National Academy in that city; in the same year he was employed as arbitrator to settle the questions in dispute between the Colonial Office and the New Zealand Company. About the same time he contested the representation of the University of Cambridge in the Liberal interest, but without success. Shortly afterwards he was appointed a member

of the Ecclesiastical Commission, and for many years was one of the Commissioners for managing the estates of the Bishops and Cathedral bodies. In 1848 he again went on a mission to Edinburgh, and reported upon the vexed questions of the Annuity Tax and the Fishery Board. In 1851 he was appointed member of a Commission to inquire into the appointments to the Civil Service of India, the other Commissioners being Lord Ashburton, Lord Macaulay, Mr. Melville, and Professor Jowett, and the result of their labours was the establishment of competitive examinations for the entry into the Indian Civil Service. In 1853 he was one of a Commission to inquire into the Inns of Court; their recommendations, however, involving an entire reconstitution of these bodies and the utilization of their great income for the purpose of legal education, have not as yet been carried out, although some stimulus was given to changes in this direction. In 1858 Sir John Lefevre was appointed member of an Executive Commission with Sir Edward Ryan, to carry out arrangements for insuring competent appointments to the Civil Service in England; for some years this was confined to requiring pass examinations as the test of competency, but later the principle of competition was introduced, of which he was always a warm supporter. For many years, in conjunction with Sir Edward Ryan, he elaborated the system of competition and carried it out universally throughout the Civil Service. He was also a member of the Commission for restoring the Standards, for the Digest of the Statutes, and for inquiry into the Endowed Schools. It was on work of this kind that his qualities of patient inquiry, quickness of insight, and powers of persuasion were so conspicuous, and led to his services being so frequently called in aid by the Government. All this work on Commissions and Inquiries was undertaken without claim for pay or reward; but in 1857, the distinction of the Order of the Bath was conferred on him. His numerous official duties did not prevent his devoting his spare hours to literary and scientific work, and even to work of other kind. He was one of the first Directors of the London and North Western Railway; he was one of the Founders of the Athenæum Club and of the Political Economy Club. He was a Fellow of the Royal Society (elected 1820) and a member of many other learned societies. In addition to his mathematical acquirements, which he maintained to the latest period of life, he was able to devote much time to literature. He had a great facility for learning languages, and, besides that of Hebrew, mastered all the languages of Europe; he commenced to study Russian after the age of 65, but even after this found time to acquire a most extensive knowledge of its literature. In 1875 he was compelled by a severe attack of illness to retire from office, but he continued to devote himself without remission to his literary studies; he died on the 20th of August, 1879, at the age of

83, after a short and painless illness, in the full possession of all his faculties, with undimmed memory and intellect, with unabated interest in public affairs, and retaining to the last that ever-ready fund of sympathy and that sweetness of character which so much endeared him to his friends and relations.

Sir Thomas Maclear, the eldest son of the Rev. James Maclear, of Newton Stewart, Tyrone, Ireland, was born on the 17th of March, 1794. At the age of fifteen he was placed under the care of relatives in England, and educated for the medical profession, studying at Guy's and Bartholomew's Hospitals. He had excellent prospects in London, but, preferring a more quiet life, he obtained the appointment of House Surgeon to the Bedford Infirmary, and at this place, in the society of the late Admiral Smyth, he had opportunities of employing himself occasionally in the practice of astronomy, a science for which he had early shown a predilection. In 1823 he moved to Biggleswade, and here erected a small observatory, in which he spent most of the time he could spare from the practice of his profession. In 1825 he married Mary Pearse, daughter of one of his Bedford friends.

In 1833, probably through the recommendation of Admiral Smyth and Sir John Herschel, he was appointed Astronomer at the Cape of Good Hope. He reached his new sphere of labour in January, 1834. Less than a fortnight later Sir John Herschel arrived at the Cape to make the survey of the southern heavens, which occupied him four years, and the two astronomers assisted each other during this period with heart and hand. The transit instrument and the mural circle were kept in constant use by Maclear and his single assistant, the result being that in the clear skies of the Cape observations accumulated far beyond the limited powers of reduction.

In the early part of 1838 he commenced his great undertaking, the verification of Lacaille's arc of the meridian, the field-work for which was completed in 1847. It has formed the basis for the survey of the colony, and has given to it a character and completeness which might otherwise have been wanting. The results of this important geodetic operation were published in two quarto volumes in 1866, under the editorship of Sir George Airy. In 1849 Maclear's means for extra-meridional observations were increased by the mounting of a 7-inch Merz refractor, he having previously been limited to the use of a Dollond achromatic of 46 inches focal length. Many valuable series of observations of comets when beyond reach at the European observatories were thus obtained, and have been of the greatest assistance in the accurate determination of their orbits. Between the years 1849—1853 the whole of the stars of the British Association Catalogue having south declination were observed by Maclear with the meridian instruments. The reduction of these observations, as of others, was much

delayed by the amount of time and attention given to work connected with the verification of Lacaille's arc, so that it has happened that the present astronomer is now engaged upon the formation of a catalogue from them for the epoch 1850.

In 1859, Maclear visited England for a few months. He returned to the Cape in the following year, and after this time his attention was mainly directed to the reduction of his previous observations. He reduced the series made in the interval 1835–40, which, after revision by his successor, Mr. E. J. Stone, has been formed into a catalogue for 1840, containing 2,892 stars. He also partly reduced the observations in the years 1856–60, a work completed by Mr. Stone, and published as the "Cape Catalogue for 1860," which contains 1,159 stars. Amongst his miscellaneous astronomical work was the determination of the parallaxes of α and β Centauri, confirming for the first of these stars the result of his predecessor, Henderson.

Maclear continued closely occupied with his astronomical duties until 1870, when he retired from the direction of the Cape Observatory, still continuing to reside in the vicinity, and taking a very lively and active interest, as he had previously done, in every matter tending to promote the well-being of the colony, in which he was greatly esteemed and respected. Latterly, his sight failed him, and for several years before his death he was quite blind. He was much interested in the exploration of South Africa; and the last occasion on which he left his house was to attend a meeting in Cape Town, when Stanley visited the colony. He died at his residence, Grey Villa, Mowbray, Cape of Good Hope, on July 14, 1879. He was elected a Fellow of the Royal Society in 1831, Member of the Academy of Sciences of Palermo in 1835, and a Correspondent of the Institute of France in 1863. For his verification and extension of Lacaille's arc, he received the Lalande Prize from the French Academy of Sciences in 1867, and a Royal Medal in 1869 from the Royal Society. Maclear was knighted in 1860.

In proof of the estimation in which Sir Thomas Maclear was held in the Cape Colony, it may be mentioned that the following resolution was agreed to in the House of Assembly on the 17th of July last:—"That this House desires to express its deep sense of the signal services rendered by the late Sir Thomas Maclear, Knt., F.R.S., F.R.A.S., to the general cause of Astronomical and Geographical Science while in charge of the Royal Observatory, Cape Town, and also to the material interests of the colony in the practical application of his researches; and, furthermore, its high appreciation of his devotion for a long period of years to the cause of South African exploration and civilization; and that this Resolution be recorded in the journals of the House."

Mr. Anthony Mervyn Reeve Story-Maskelyne was born May 8th, 1791. He was descended from the family of Story, who were settled at Know, in the county of Cumberland, but being Jacobites were obliged to leave that part of the country after the Rebellion of 1715. Mr. Story-Maskelyne's grandfather settled at Oxford soon after 1715, and died there in 1751. He had a son, William Story, who was born in 1734, and became rector of Hinton-Martell, county Dorset, on the presentation of the Earl of Shaftesbury. This gentleman was the father of the subject of the present memoir. Mr. Story-Maskelyne lost his father early in life, but his mother survived her husband till 1827. The care and education of her son fell to her, and Mr. Story-Maskelyne often spoke with pride and gratitude of her devotion and judicious management. When he went up to Oxford and entered at Wadham College he found himself far behind the scholarship of the place, but with a spirit for work and a passion for knowledge with which his mother had inspired him, he would tell with some humour of his first essay for a scholarship, when, on a friend inquiring "how he had done," the answer was that he had made four false quantities in the first two lines of an ode of Horace. But this remark was the spur to that undaunted effort which resulted in the remarkable success of a "double-first." When Mr. Story-Maskelyne took his "double-first," in 1810, aged just nineteen, only three men before him had won that honour, Sir Robert Peel, Mr. C. Bathurst, and the Rev. J. Keble; and when we remember his imperfect preparation for Oxford and his final success, we can only be astonished at the labour and the ability which were required and expended to achieve it. The late Warden of Wadham, Dr. Symons, was his tutor, and may well share the praise of such work. Mr. Story-Maskelyne, having selected the bar as his profession, was called in 1816, and went on the Western Circuit for five or six years. He was a elected a Fellow of the Royal Society in 1823. Tempting offers came to him afterwards. By his degree, he had made himself a man of mark, and among others the Lord Shaftesbury of that day proposed to him to bring him into Parliament for one of those boroughs which, before the Reform Bill, were at the command of their "owners." This Mr. Story-Maskelyne declined, for the acceptance of the offer would have bound him to a party—perhaps almost to the opinions of his patron, and it was the characteristic of his mind then, and till the last, to be the independent master of his own opinions. But it is most probable that his marriage with Miss Margaret Maskelyne, the only child of the Rev. Dr. Maskelyne, F.R.S., Astronomer Royal, gave the turn and character to his career which continued to the end. Mr. Maskelyne's marriage took place in 1819, and the ancient patrimony of the Maskelynes at Purton and Bassett Down came with his wife, as sole heiress, to him.

But perhaps the withdrawal of Mr. Maskelyne from all public and county business may have another explanation, at least supplementary to that to which we have here ascribed it. His tastes were those of an educated and well-read gentleman. To him the treasures of the great minds of Greece and Rome were as completely open as are those of our own language to us, and we know that to within a very few years of his death he still enjoyed the beauty and wisdom laid up in their works, and found his rest and enjoyment there. Perhaps with such tastes and such occupations, a want of sympathy from others or a want of congeniality of taste in others, led him to separate himself from those useful and honourable men to whom this country owes so much for their care of our national and county affairs.

Mrs. Story-Maskelyne died in 1858, so that her husband has survived her twenty-one years. She inherited from her father much of his great ability, and was enabled to correct the press for the last publications he wrote. But her kindly heart, and a charity that checked all expression, all feeling of an unkind thought, are the great charms by which she is best remembered now.

Sir James Matheson, Baronet, was the second son of Captain Donald Matheson, of Shiness, in the county of Sutherland, and was born in 1796. He was educated at the High School and University of Edinburgh, and after a few years' business training in London, he went out to the East, and engaged in mercantile pursuits. He was one of the founders of the eminent house of Jardine, Matheson, and Co., in China. In 1836, during a visit to this country, he published a volume on the China trade, which threw light on a subject not then much understood. In 1842, he finally retired from business with an ample fortune, and in the following year married Mary Jane, daughter of Mr. Michael H. Perceval, a member of the Legislative Council of Quebec.

Having acquired the estate of Achany in his native county, Mr. Matheson in 1844 purchased from the Seaforth family the Island of Lewis, in the Hebrides, and made it for the most part his home. It was his earnest desire to improve the condition of the large population on this estate, and with large-hearted and liberal plans he entered upon his self-imposed task, employing the most able assistants whom he could find to carry out his projects. Large outlays were made in the construction of roads and bridges over the whole island, in draining and reclaiming waste land, in establishing steam communication with the south, and in promoting education. Scarcely had he begun these various works when, in common with Ireland, the west Highlands were visited by the potato disease, and many thousands of persons on his property were threatened with famine. His untiring

efforts at that time, and the munificent provision which he made for meeting the distress, can never be forgotten. They were not unobserved by Her Majesty's Government, and in recognition of his generous conduct he was created a baronet.

Sir James entered Parliament in 1843, as member for Ashburton, and in 1847 he was returned for the combined counties of Ross and Cromarty, for which he continued to sit until 1868. He was elected a Fellow of the Royal Society in 1846. He died at Mentone on the 31st December last, aged 82. Lady Matheson survives him.

John Miers was born in London on the 25th of August, 1789. He entered his father's business as a jeweller, but evincing a decided inclination for scientific research, devoted himself in his leisure hours to the study of the physical sciences, and especially of chemistry and mineralogy. At this early period of his life he undertook a series of experimental researches in the endeavour to demonstrate the compound nature of azote, which resulted in the publication of two papers on this subject in Thomson's "Annals of Philosophy," in the year 1814; he also took a leading part in the foundation of the "Philosophical Society of London"—an institution with which many young men who in after years distinguished themselves in science were connected. At this time he made the acquaintance of Michael Faraday.

In 1818 he married, and shortly after, at the invitation of his friend. Lord Cochrane (afterwards Earl Dundonald), proceeded to Chili to assist in the erection of extensive machinery for the reduction and manufacture of copper; an enterprise in which both were interested. Landing at Buenos Ayres, he traversed the Pampas and the Cordilleras, and, during six years' residence in the country, devoted his leisure time to the study of its Flora and Fauna. Although at this time unacquainted with the elements of botany, he gradually acquired a practical knowledge of the science by numerous dissections and observations upon the plants of Chili in the living state. He returned to England in 1825, and during a stay of a few months obtained personal introductions to Robert Brown and Dr. Lindley, who encouraged him in his studies. In 1826, shortly after his return to South America, was published his "Travels in Chili and La Plata," in two volumes, a work which was for long regarded as a standard authority on the geography of the countries and customs of the people with which it dealt. He twice again traversed the Pampas, and afterwards resided for seven years in Rio de Janeiro, where he had contracted to erect a Mint for the Brazilian Government, and where, although incessantly engaged in his professional duties, he added largely to his botanical and entomological collections.

Mr. Miers returned to England finally in 1838, and from this period

the commencement of his scientific career may be dated. He contributed numerous papers on the botany of South America to the "Transactions" and "Journal" of the Linnean Society, the London "Journal of Botany," and the "Annals and Magazine of Natural History." He also published several larger works, which although consisting in great part of reprints of his smaller memoirs, are enriched with many additional observations, and with numerous plates executed from his original drawings, and many of them drawn by himself on the stone. His "Illustrations of South American Botany" appeared in 1850, and his "Contributions," in three volumes, followed in 1867, 1869, and 1871; the third and concluding volume of the latter work is occupied exclusively with his "Monograph of Menispermaceous Plants," and is the most valuable of his contributions to science. His latest volume, a "Memoir on the Apocynaceæ," was published in 1878, when its author was in his 89th year.

Mr. Miers was elected F.L.S. in 1839, and F.R.S. in 1843, and was a member of several foreign learned Societies. He served as Juror in the Brazilian Section of the International Exhibition of 1862, and for his services the Emperor conferred upon him the Cross and Grand Cross of the Imperial Order of the Rose.

As a botanist, Mr. Miers was distinguished by the extreme carefulness and accuracy of his observations and descriptions, and the great beauty and fidelity of his drawings and analyses. He was to the last an opponent of the theory of Evolution. In private life he was ever regarded with affection and esteem by all who knew him.

In June, 1879, he was compelled by failing health to desist from active work, and after several months of ever-increasing infirmity, borne with unfailing patience, breathed his last on the 17th October, 1879, in the 91st year of his age.

His extensive botanical collections have been bequeathed to the British Museum.

Charles Murchison was born in Jamaica in 1830. Some time afterwards, his father, who belonged to a Scotch family, and was nearly related to Sir Roderick Murchison, left the West Indies, where he had been practising as a physician, and returning to his native land went to reside at Elgin. At the age of fifteen Charles Murchison went to the University of Aberdeen, and two years afterwards began the study of medicine in Edinburgh University. Even as a student he displayed the same qualities which characterised him in after life, manual dexterity, retentive memory, clearness of thought, and plainness of exposition, combined with great energy and industry. These qualities not only enabled him to gain many distinctions in the classes he attended, and to take his degree with the highest honours,

but they obtained for him so much esteem amongst his fellow students as to lead to his election as one of the Presidents of the Royal Medical Society. Before taking his degree he had obtained the Licentiateship of the Royal College of Surgeons, Edinburgh, and had acted as House Surgeon for Mr. Syme. After his graduation, at the age of twenty-one he became Physician to the British Embassy at Turin. In the following year he returned to Edinburgh and acted for a while as resident Clinical Physician in the Infirmary. After resigning this appointment he studied for a short time in Dublin and Paris. He then entered the Honourable East India Company's service, and soon after his arrival in Calcutta, was appointed Professor of Chemistry in the Medical College. During the Burmese War he was attached to the Army Medical Staff, and in 1855 he returned to London. Very soon afterwards he was appointed Lecturer in Botany at St. Mary's Hospital. He was subsequently connected with King's College Hospital, the Middlesex, St. Thomas's Hospital, and in the latter two schools held the appointment of Lecturer on Medicine.

In addition to these appointments, he held for many years that of Assistant Physician and afterwards of Physician to the London Fever Hospital, an office which demanded much time as well as much labour.

The subjects to which Murchison devoted his attention as an original observer were botany, pathology, and medicine. Botany was the subject which he first took up. He began to study it while in Aberdeen, and prosecuted it vigorously in Edinburgh under Professor Balfour, in whose class he obtained the medal for the best herbarium. Three years afterwards he contributed his first original paper on "Glandular Organs in Plants" to the Botanical Society of Edinburgh. As his attention became directed to other subjects, his botanical studies were more or less interrupted. He continued them in Burmah and again resumed them when he became Lecturer on Botany to St. Mary's, at which time he contributed several original papers to various journals, but they were almost entirely discontinued after he ceased to lecture on botany, his powers then becoming more concentrated on the subjects of pathology and medicine.

His first original pathological observations were made during his tenure of office as House Surgeon under Mr. Syme, and were embodied in his thesis on "The Pathology of Morbid Growths," for which he obtained a gold medal at his graduation.

From the time of his connexion with St. Mary's Hospital, his interest in pathology remained undiminished up to the time of his death. His contributions to the "Transactions of the Pathological Society" were very numerous, and his services to this subject were recognised by his appointment, in 1877, as President of the Pathological Society. His most valuable contributions to medicine were his works on "The

Continued Fevers of Great Britain," and on "The Functional Diseases of the Liver."

The opportunities for observation which his position as Physician to the London Fever Hospital afforded, were used by him so diligently that he was able in this work to lay down the distinctions between the various continued fevers as regards their causes, symptoms, and pathological conditions with great clearness and accuracy; and thus he contributed in no inconsiderable degree to the general acceptance of the views now held regarding the etiology of typhoid fever, and which have already been of such service in regard to its prevention.

His work on "Disease of the Liver" was characterised, like all his other writings, by such careful observation of facts and accuracy of statement, as to render it a most useful aid in the diagnosis of the diseases of which it treated.

As a teacher of clinical medicine, Murchison has left few equals in this country. The system he followed was modified on that of his old teacher, Dr. Hughes Bennett. The essential part of the plan was first to excite the attention of the students by skilfully putting questions, and then to satisfy the desire for information which he had thus awakened, instead of at once stating to them facts in which they took no interest. The extent of his knowledge and his methodical habit of mind, combined with the admirable plan of teaching which he adopted, left a deep impression on the students whom he taught, and his personal influence will long be felt in his writings. He was elected a Fellow of the Royal Society in 1866.

Arthur Hay, 9th Marquis of Tweeddale, President of the Zoological Society of London, was born on the 9th November, 1824, the second son of his father, the previous Marquis, who was a Field-Marshal, and a well-known Peninsular veteran. In April, 1841, Lord Arthur Hay, as he was then called, obtained a commission in the Grenadier Guards, and, on obtaining the rank of Captain in 1842, proceeded to India as Aide-de-camp to his father, who was at that time Commander-in-Chief at Madras. It was at this period of his life that Lord Arthur Hay made the acquaintance of the late Dr. Jerdon, the distinguished Indian naturalist, who was then Assistant-Surgeon at Fort St. George. The two saw much of each other, and worked together at various branches of natural history; and the friendship thus formed was renewed when Dr. Jerdon retired from the Service and returned to England in 1869. At the commencement of the Sutlej Campaign of 1845–46 Lord Arthur Hay was appointed Aide-de-camp to Lord Hardinge, the Commander-in-Chief, and was present on his Staff at the decisive battle of Sobraon. On the 9th March, 1846, the Treaty was concluded by which the whole of the hill territory west of the Sutlej and

Cashmere was handed over to the British; and a few days afterwards the second Treaty, by which the Kashmir portion was transferred to the Maharajah Golab Singh, was executed. Shortly after this, Lord Arthur Hay, with other officers, among whom were Lord Elphinstone, Lord James Brown, Captain H. Bates, and Lieutenant A. Hardinge, obtained leave to visit this part of the Himalayas; and, after being received with marked courtesy by the Maharajah at his ancestral residence of Jummoo, they crossed by the Banihal Pass into the valley of Kashmir. After spending a month there, Lord Arthur Hay and Lord Hardinge started for Ballastan, or Little Tibet, viâ the Kishengunga Valley and the Dessai Plains, and, after visiting Skardo, travelled on to Leh, in Ladakh, and thence through Rupshu to cross the high pass, the Parang La (19,000 feet), finally ending their tour at Simla. The party met with a good deal of trouble and vexatious delay in this latter part of the journey, which at that period was of a venturesome, if not of a dangerous nature. Lord Arthur Hay was, throughout this journey, engaged in his favourite study, and made a large collection of the birds of the country.

During his stay in India, Lord Arthur Hay, although much interested in natural history, and on terms of intimacy with Dr. Jerdon, Mr. Blyth, Sir Walter Elliot, and other Indian naturalists, gave very little of the results of his studies to the public. We can only find published during this period the two articles in the "Madras Journal," which stand at the head of the subjoined list.

During the next period of his life, Lord Arthur Hay, who assumed the title of Viscount Walden in 1862, on the death of his elder brother, Lord Gifford, was too much engaged with his military duties and other matters to be able to do much scientific work. In 1854 he accompanied the army sent out to Turkey, and thence to the Crimea, and took part in the campaign which resulted in the fall of Sebastopol. In 1866 Lord Walden finally retired from active service, and commenced anew a collection of birds and ornithological books in a house which he built for himself at Chislehurst, and which was for the ten following years his habitual residence. He became a frequent contributor to "The Ibis," to the Zoological Society's "Proceedings" and "Transactions," the "Annals of Natural History," paying special attention to the birds of India and the Eastern Archipelago. In 1868, Lord Walden, upon the death of Sir George Clerk, was elected President of the Zoological Society of London, of the Council of which he was already a member, and retained this office, discharging the duties with the greatest zeal and success, until his death. He was elected a Fellow of the Royal Society in 1871.

On the death of his father, at a very advanced age, in 1876, Lord Walden succeeded to the peerage and estates, and transferred his home and collections to the ancestral seat of Yester, in East Lothian,

where he subsequently passed the greater part of his time. During the past two years Lord Tweeddale devoted special attention to the investigation of the avifauna of the Philippine Archipelago. Mr. A. H. Everett, a well-known collector, was specially engaged to visit the different islands of this group, and make collections of their birds; and the results were given to the public in a series of thirteen papers published in the Zoological Society's "Proceedings," the last of which was finished only a few days before the death of the author.

Another important piece of ornithological work recently undertaken by Lord Tweeddale was the editing, with notes and additions, of the part relating to the birds of "Blyth's Catalogue of the Mammals and Birds of Burma," which was left in a very unfinished state at the author's decease. Under Lord Tweeddale's "able and conscientious treatment," to use the words of Mr. Grote, the catalogue became a complete list of all the known Burman species, and a most useful work to ornithologists.

Lord Tweeddale died at his English residence, Walden Cottage, Chislehurst, on the 29th December, 1878, after a very short illness. His collections of birds and scientific books are bequeathed to his nephew, Mr. Robert Wardlaw Ramsay, a well-known member of this Union.

John Waterhouse was born at Halifax, Yorkshire, on the 3rd August, 1806.

Very early in life he evinced a decided preference for scientific studies; his school training increased this preference and enabled him to obtain a sufficient knowledge of mathematical science, which he turned to good account in after years in various branches of physical research.

A certain weakness of constitution which prevented him in his youth from great bodily exertion, only seemed to stimulate his mental activity; and when in search of change of climate, with a view to invigorated health, he undertook a voyage round the world, his acquirements enabled him to keep a journal abounding in facts and observations which, had his modesty not shrunk from appearing in print, would have proved the record of a "Scientific Expedition." During the voyage, his love of nature, and the wide range of his scientific tastes acquired an increased stimulus; and when he returned home his experience in observation, and his knowledge of natural phenomena in different parts of the world, enabled him to enter with renewed pleasure into the less active study of the physico-mathematical sciences.

He established an astronomical and meteorological observatory, and in 1874 published a quarto volume on the "Meteorology of Halifax," which will continue to be a model for all such local observations.

Practical botany also engaged his attention, and his gardens were distinguished throughout the neighbourhood for the rich variety of their contents, especially in rare plants and exotics, of which he was justly proud.

His favourite studies were astronomy, geology, electricity, and light. He took part in the early progress of photography, and in the discovery by the Rev. J. B. Reade, F.R.S., of the method of taking portraits first upon leather, and afterwards upon paper, instead of silver plates or glass; and also with the chemical means of giving permanence to such images. He was an adept in working with the microscope, was extremely fond of music, and a good player on the violoncello, while in practical applications few men could handle their tools better than he, for in addition to his scientific acquirements he was a good mechanician, and many of his turnings in ivory were almost Chinese in their beauty and skilful execution.

Those who knew him best can best appreciate the many-sided features of his genius, and will long remember the evenings spent at Well Head, where, although reticent in public, he would converse with ease upon the most abstruse departments of mathematical or physical investigation with an originality of illustration which showed that he was practically, as well as theoretically, acquainted both with the facts and principles of science. He interested himself actively in the spread of scientific knowledge, was an early supporter of the Halifax Mechanics' Institute, and delivered lectures to the local Literary and Philosophical Society, of which he was one of the founders and for many years the President. He also enriched the museum with many choice objects of natural history, collected during his travels. His disposition was generous, and needy investigators were on many occasions assisted by his bounty. As a magistrate he was for many years Chairman of the County Bench at Halifax, and a Deputy-Lieutenant for the West Riding, whereby he proved himself a worthy descendant of a family which for four hundred years had been intimately associated with the prosperity of the town and neighbourhood. In later years a stroke of paralysis, which compelled his retirement into private life, only made him appreciate the more his beautiful gardens, until a severe form of the malady prevented all mental occupation, and finally terminated his life on the 13th February, 1879, in the 72nd year of his age.

He was elected into the Royal Society in 1834, and was a Fellow of the Royal Astronomical, the Royal Microscopical, the Geological and other Societies.

Ernst Heinrich Weber, the third child of Michael Weber, Professor of Positive Divinity at Wittenberg, was born on the 24th of June, 1795. He was a vigorous, cheerful boy, fond of active sports as

well as of study, and made rapid progress at the Fürstenschule at Meissen, and in his sixteenth year left school to begin his studies at the University of Wittenberg. About this period he contracted a friendship with the celebrated Chladni, who was a frequent guest at his father's house, and this intimacy lasted as long as Chladni lived.

Weber's studies at Wittenberg soon met with a serious interruption. The estates from which the University derived its revenue were laid waste in the campaign of 1813; then followed the siege of Wittenberg; and, at length, when in January, 1814, the town fell into the hands of the Prussians, the University itself was removed to Schmiedeberg for two years. On this migration to Schmiedeberg, Weber went to Leipzig, where he worked under the anatomist Rosenmüller, and through Chladni's introduction, was brought into scientific intercourse with the physicist Gilbert. He went rapidly through the various medical courses, and soon passed his examination in comparative anatomy. On the 6th of June 1815, he took the degree of M.D. at Schmiedeberg. Before he had completed his medical studies the fate of the University of Wittenberg was decided with that of Saxony. On its abolition, his father and five other Professors accepted a call to Halle; but Ernst Heinrich chose rather to go to Leipzig, which even then, though only containing 25,000 inhabitants, ranked as one of the great cities of Germany. Here he entered on medical practice as assistant to Professor Clarus, without however giving up his favourite pursuits. He had already, in 1817, completed his "Anatomia Comparata Nervi Sympathici," which he sent in as his inaugural dissertation. The explanatory plates to this work he himself engraved on copper. The publication of this book at once brought his name into prominence, and he shortly afterwards received the offer of an Extraordinary Professorship of Anatomy at Bonn; but a similar appointment being offered him at Leipzig, he chose the latter.

In addition to the performance of the duties of the chair, he went on with his scientific investigations, amongst which was a research on the organ of hearing of fishes—in the course of which he made several discoveries. In 1821, on the death of Professor Rosenmüller, Weber became Ordinary Professor of Human Anatomy at Leipzig. About this time he married the sister of his friend Schmiedt, and with her lived happily for upwards of fifty years. It was at this period that, in conjunction with his brother, Wilhelm Weber, he entered on the classical researches which led to the establishment of the celebrated theory of waves. In company with his wife and his brother Wilhelm, he made an excursion amongst the Alpine lakes in order to observe the motions of waves on a large scale. The observations made in this journey led to his making a series of experiments in his father's house; and for four years Weber walked regularly every week to Halle and back for the purpose of prosecuting this investigation. Among the most

remarkable experiments might be mentioned those on waves of water in troughs, by means of which he found that the particles near the surface move in circular paths, while those deeper in the liquid describe ellipses, the horizontal axes of which are longer than the vertical. By another series of comparative observations on water and mercury, the law was established that waves move with equal rapidity on the surfaces of different mediums, while the rapidity increases in both cases with the depth of the liquid. These and a multitude of other facts have been of the utmost value in relation to the theory now accepted in explanation of the phenomena of light and sound.

This important research was followed by other observations and results published in various papers, as, for example, that entitled "Ausdehnbarkeit und Elasticität der Haarë." This latter led him to the discovery of the so-called tension of organic substances. In the paper "Ueber einige Einrichtungen der Wirbelsäule" ("On some Peculiarities in the Mechanism of the Vertebral Column") he showed how much the study of the structures of organs is aided by their being observed during movements resembling those occurring in life, as well as when in a state of rest.

In 1830 appeared his edition of Hildebrandt's "Anatomie," of which he wrote anew a considerable part. In that work he examined carefully into the trustworthiness of the methods usually adopted in anatomical investigation, especially the use of the microscope, at that time a very imperfect instrument. As he carefully separated theories from established facts, he succeeded in arranging everything in its proper place, and showed the meaning of many experiments which his predecessors had passed over in silence.

It was at the time of the appearance of the "Handbuch der Allgemeinen Anatomie" that the political calm which had prevailed since the fall of Napoleon came to an end. About this time the Saxon Government took occasion to make considerable alterations in the constitution of their Universities, but unluckily limited themselves to changes in the administration of the funds, overlooking the necessity for providing such means for the Professors as would admit of their giving themselves up entirely to their calling. The excitement caused by these changes in the old statutes was great, but happily only of short duration, for they were set aside by the events which led to a democratic constitution of the State. When the new constitution was established, and the Chambers again met in order to arrange the various details of the administration in accordance with the new principles, the affairs of the University soon came under discussion. The general feeling towards the institution was decidedly favourable, as was to be expected from the liberal tendency of the time, and most fortunately for the University, it was represented in the Upper House by E. H. Weber. His views on University affairs were presented to the

Lower House in the form of a memorial, and these views had all the more weight from the fact that, instead of pressing the urgent need of more space for his own department, he insisted specially on measures of more general usefulness, such as the establishment of a library and the arranging of a collection of casts. The second Chamber cordially voted to the University 10,000 dollars, in addition to the sum originally proposed by Government.

The city of Leipzig soon had reason for joining in the approbation of his colleagues when he succeeded in preventing the increase of taxation which it had been contemplated to impose on the mercantile class, and he finally won the hearts of all Protestant Saxony by the manly, yet temperate language which he made use of in the discussions on the subject of religious education. On his return to Leipzig the citizens received their delegate with great demonstrations of respect, and he was even conducted into the city through a triumphal arch.

Weber took an active part in the establishment of the Polytechnic Society, and of the Scientific Society of Leipzig, as well as of the Professorium. He instituted microscopic work in the University, and, in conjunction with the medical faculty, introduced improvements into the medical examinations.

After the death of Kühn he undertook the regular discharge of the duties of the chair of Physiology.

In 1850 he followed up his research on the theory of waves by an elaborate series of experiments, designed to study the wave movement in the arterial system, and explain the fact that the pulse-beat is felt in the chin a fraction of a second sooner than in the foot. The results showed that the pulse-beat travels with a rapidity of about thirty-five feet per second, and that in general the rapidity of a wave in small elastic tubes is not affected by the increase of pressure on the walls. The results obtained by these experiments were published in the treatises "De Pulsu Arteriarum,' "De Utilitate Parietis Elastici Arteriarum," and, above all, in that entitled "Die Anwendung der Wellenlehre auf den Kreislauf des Blutes." These experiments gave a new direction to the study of the subject of blood dynamics. Weber subsequently explained the difference between absorption by the veins and absorption by the lymphatics, and on another occasion he was able to determine with certainty, the manner in which the excretory ducts of racemose glands end, and the relation between their alveoli and the blood current.

Weber also made a series of most careful observations on nerve trunks, and published their results in a series of treatises beginning with that entitled "De Subtilitate Tactus," and ending with "Die Lehre vom Tastsinn und Gemeingefühl für Aerzte und Philosophen." He also published some interesting results of experiments on the mechanism of the ear, as well as on the microscopic phenomena visible

on bringing together alcohol and resin suspended in water in capillary spaces. In 1851 appeared his book "Annotationes Anatomicæ et Physiologicæ," a work which contains accounts of his most valuable investigations.

Weber lived in intimate friendship with his two brothers—Wilhelm, his fellow worker on the theory of waves, and a younger one, Edward, who also settled in Leipzig. He lived to the advanced age of 83, and died on January the 26th, 1878, at Leipzig.

E. H. Weber was elected a foreign member of the Royal Society in 1862.

In Bennet Woodcroft the Society loses a man who occupied himself rather with the application of scientific principles to practical purposes than with theoretical investigations. He was born at Heaton Norris, Stockport, on the 29th of December, 1803, and in due time entered into partnership with his father, who carried on the business of a dyer and velvet finisher at Salford. Previous to this he had acquired a practical knowledge of the art of weaving, and had also studied chemistry under Dalton, of whom he became a favourite pupil. About 1843 he commenced business in Manchester as a consulting engineer, and shortly afterwards removed to London. In April, 1847, he was appointed Professor of Machinery at University College, which post he held until July, 1851. He was not a successful lecturer, for although his views were clear and decided, and he possessed the mechanical faculty in a high degree, he lacked the power of sustained exposition necessary to a successful lecturer. Upon the passing of the Patent Law Amendment Act in 1852, Woodcroft was appointed by Lord Cranworth, then Lord Chancellor, to the post of Superintendent of Specifications, a somewhat anomalous title which in no way indicated the responsible nature of the duties he was afterwards called upon to perform. On the retirement of Mr. Edmunds, he became, August 1st, 1864, Clerk of the Commissioners, which placed him in sole control of the department. He retired from the public service March 31st, 1876, and his death occurred February 7, 1879, at his house in Redcliffe Gardens.

In many respects it would have been difficult if not impossible to have found a better man to place at the head of the Patent Office than Mr. Woodcroft. Himself an inventor, he had an extensive acquaintance with inventors; he knew their peculiar needs, and he possessed an intimate knowledge of the patent laws. His name will always be associated with the great work of printing and indexing the patent specifications—a series of records which present an unbroken view of the industrial progress of the nation for more than two hundred and fifty years. He displayed the keenest interest in all that related to inventions, and he spent much time in clearing up doubtful points in

the history of various arts, especially those of steam navigation and the manufacture of textile fabrics. The results are partly embodied in the "Sketch of the Origin and Progress of Steam Navigation" (1848), and "Brief Biographies of Inventors of Machines for the Manufacture of Textile Fabrics" (1863). Very little of the material collected during the later years of his life has, however, been given to the world. The peculiar bent of his mind showed itself also in the formation of the Patent Office Museum, which was first opened to the public in June, 1857. The collection is small, but it embraces a number of machines and models of the highest historical interest, many of which would have been destroyed or suffered to fall into decay but for Mr. Woodcroft. Forming part of the Museum, was a collection of portraits of inventors and authors of discoveries in connexion with the useful arts. A considerable portion of his private fortune was expended in the purchase of portraits and objects for the Museum, but partly in consequence of the want of a proper building, the Museum has not attained that importance which was hoped for by its founder. The late Prince Consort took a warm interest in the scheme.

Mr. Woodcroft's inventions embraced subjects connected with weaving, marine propulsion, and dyeing. His "tappet" for looms, which was patented in 1838, is now in universal use in looms for weaving a certain class of goods. From an early period in his life he had paid great attention to the subject of marine propulsion, and in 1832 he obtained a patent for his well-known increasing pitch screw propeller, which Lord Brougham, in the course of a judgment in the Committee of Privy Council, spoke of as "a very refined and ingenious contrivance." In all the screws previously suggested the blade had a uniform pitch or incline, but Woodcroft's screw is formed by a blade so coiled round a shaft or cylinder that the angle of inclination of the worm with the axis continually decreases, and the pitch or distance between the coils continually increases throughout the whole length of the shaft upon which the spiral is formed. In consequence of this elongation of pitch each successive part of the spiral begins to act before it is overtaken by the current given to the water by the action of the preceding part, and consequently every part meets with resistance from the water. In this way a proportion of propelling power is gained and loss from slip is avoided. He also patented a "varying pitch" screw propeller, the object of which was to enable the angle of the blades to be altered at pleasure, according to the varying circumstances of wind, currents, or tonnage. In the course of his business as a dyer, he had been struck with the difficulty of printing calico with indigo, in consequence of the rapid oxidation which takes place, and he made a series of experiments on the practicability of carrying on the operations in an atmosphere of

coal gas. The results were embodied in two patents, granted respectively in 1836 and 1846. Although a considerable saving of indigo was effected and much better work produced, the process was not a commercial success.

He was elected a Fellow of the Royal Society in 1859.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

May 1, 1879.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

Prof. John Anderson, M.D.
Rev. Miles Joseph Berkeley, F.L.S.
Henry Bessemer.
Prof. Alexander Crum Brown, M.D.
Walter Lawry Buller, Sc.D., F.L.S.
George Howard Darwin, M.A.
Prof. Joseph D. Everett, M.A., D.C.L.
Prof. Francis Stephen Bennet François de Chaumont, M.D.
Prof. George Downing Liveing, M.A.
George Matthey, F.C.S.
George John Romanes, M.A., F.L.S.
Arthur Schuster, Ph.D., F.R.A.S.
Prof. Harry Govier Seeley, F.L.S.
Benjamin Williamson, M.A.
Thomas Wright, M.D., F.G.S.

The following Papers were read:—

I. "A Magnetic Survey of the Fortieth Parallel in North America between the Atlantic Ocean and the Great Salt Lake, Utah." By T. E. THORPE, Ph.D., F.R.S., Professor of Chemistry, Yorkshire College, Leeds. Received March 25, 1879.

(Abstract.)

This communication contains the results of a series of observations of the three magnetic elements—dip, intensity, and declination—made

along the 40th parallel in North America between the Atlantic Ocean and Salt Lake City. Magnetic observations have been made, with more or less assiduity, at different places in the eastern States for many years past; but of the immense tract of country lying between the Mississippi and the Pacific Ocean there is only a single determination of one of the three elements indicated on Sir Edward Sabine's maps, viz., a determination of declination at Salt Lake City. A series of observations was made some years since by United States' officers along the Mexican frontier, and a similar series was carried out by the English and American officers employed on the North American Boundary Commission. The present set of observations was made, therefore, along the district which lies midway between the line of observations already run along the northern and southern boundaries of the United States' territory.

The instruments employed formed part of the admirable magnetic equipment belonging to Owens College, Manchester, and were kindly placed at the disposal of the author by the Council of the College. At the commencement and end of the survey a complete series of observations was made at Kew, which was adopted as a base station; and a similar set was made at Washington in the magnetic observatory belonging to Mr. Charles A. Schott, of the United States' Coast Survey Department, with a view of obtaining an independent check on the indications of the instruments. The author learned from Mr. Schott that the Coast Survey Department is gradually accumulating data for a discussion of the magnetic history of the American continent. It has already published a map of declination for 1870, from which it is evident that much has been done in the determination of this particular element since the date of Sir E. Sabine's memoirs, more especially along the Pacific slope, and in the regions to the west and south-west of the Great Salt Lake. As yet, however, no observations have been published relating to any of the districts west of the Mississippi visited by the author.

II. "On certain Definite Integrals occurring in Spherical Harmonic Analysis, and on the Expansion in Series of the Potentials of the Ellipsoid and of the Ellipse." By W. D. NIVEN, M.A., Fellow of Trinity College, Cambridge. Communicated by J. W. L. GLAISHER, M.A., F.R.S. Received April 3, 1879.

(Abstract.)

1. The object of this paper is to explain a general method of calculating a class of integrals connected with the expansion of functions in spherical harmonic series.

The basis of the method is expressed by the evaluation of the integral

$$\iint e^{\alpha x+\beta y+\gamma z}dS$$

taken over the surface of a sphere of radius R, whose centre is the origin of co-ordinates. This is easily shown to be

$$4\pi R^2\left\{1+\frac{R^2}{3!}(\alpha^2+\beta^2+\gamma^2)+\ldots+\frac{R^{2i}}{2i+1!}(\alpha^2+\beta^2+\gamma^2)^i+\ldots\right\}.$$

Hence it follows that, if V be any function which can be expressed symbolically in the form

$$e^{x\frac{d}{dx}+y\frac{d}{dy}+z\frac{d}{dz}}V_0,$$

where the differential coefficients of V belong to the origin, and (x, y, z) is any point within the sphere, then

$$\iint V dS=4\pi R^2\sum_0^\infty\frac{R^{2i}}{2i+1!}\left(\frac{d^2}{dx}+\frac{d^2}{dy}+\frac{d^2}{dz}\right)^i V_0 \quad \ldots\ldots\ldots (1).$$

If we next consider a circle in the plane of x, y, whose centre is the origin, and whose radius is R, we find, in like manner,

$$\int V ds$$

taken round the perimeter, equal to

$$2\pi R\sum_0^\infty\frac{R^{2i}}{2^{2i}i!\,i!}\left(\frac{d^2}{dx}+\frac{d^2}{dy}\right)^i V_0 \quad \ldots\ldots\ldots\ldots\ldots (2).$$

The theorems (1) and (2) are applied in a variety of cases, of which we will quote two results:—

Let $P_n(\mu)$ be the zonal harmonic of the nth degree, having its pole in the axis of z, and let m and n be integers, then

(1) $$\iint(1-\mu^2)^m P_{2n}dS=(-1)^n\frac{4^m}{2m+2n+1!}\frac{m+n!\,m!\,m!\,2n!}{m-n!\,n!\,n!}4\pi R^2,$$

provided m be not less than n. In all other cases the integral vanishes, the cases where the harmonic is of odd degree included.

(2) $$\iint(1-\mu^2)^{m-\frac{1}{2}}P_{2n}dS=$$

(α) $n>m$, $$(-1)^m\frac{2m!\,2n!\,2n!\,2n-2m!}{2^{2m+4n}n+m!\,n-m!\,m!\,m!\,n!\,n!}2\pi^2R^2.$$

(β) $n<m$, $$(-1)^n\frac{2m!\,2m!\,2n!\,m-n!}{2^{2m+4n}m+n!\,2m-2n!\,m!\,m!\,n!\,n!}2\pi^2R^2.$$

In the "Proceedings of the Royal Society," vol. xxvii, pp. 63—71, Professor J. C. Adams has found the value of the integral

$$\iint P_p P_q P_r dS.$$

Mr. Ferrers had also independently succeeded in the evaluation, and had published the result in his "Treatise on Spherical Harmonics." In the paper of which this is an abstract, the same integral is discussed, and an attempt is made to extend the result to the cases of three tesseral harmonics, and of four zonals with coincident poles, but with only partial success.

2. If we seek to apply the same methods to ellipsoids and ellipses, we find that

$$\iint V p dS,$$

taken over the surface of the ellipsoid of semi-axes a, b, c, where p is the perpendicular from the centre upon dS, is equal to

$$4\pi abc \sum_{\circ}^{\infty} \frac{\left(a^2\frac{d^2}{dx}+b^2\frac{d^2}{dy}+c^2\frac{d^2}{dz}\right)^i}{2i+1!} V_0,$$

and that

$$\iiint V dx\, dy\, dz = 4\pi abc \sum_{\circ}^{\infty} \frac{1}{2i+3} \frac{\left(a^2\frac{d^2}{dx}+b^2\frac{d^2}{dy}+c^2\frac{d^2}{dz}\right)^i}{2i+1!} V_0.$$

As a particular case, let us consider V as the potential due to unit matter at the point (f, g, h) outside of the ellipsoid. Then, since

$$PQ^2=(f-x)^2+(g-y)^2+(h-z)^2,$$

and therefore

$$\frac{d}{dx}\frac{1}{PQ}=-\frac{d}{df}\frac{1}{PQ},$$

$$\frac{d^2}{dx}\frac{1}{PQ}=\frac{d^2}{df}\frac{1}{PQ}, \text{ \&c.,}$$

it follows that we may take x, y, z, zero before differentiation, and write the two results:—

$$\iint \frac{pd\,S}{PQ} = 4\pi abc \sum_{\circ}^{\infty} \frac{\left(a^2\frac{d^2}{df}+b^2\frac{d^2}{dg}+c^2\frac{d^2}{dh}\right)^i}{2i+1!} \frac{1}{\sqrt{f^2+g^2+h^2}}, \quad \ldots\ldots (3)$$

a relation giving the potential of an ordinary ellipsoidal shell in spherical harmonics;

$$\iiint\frac{dx\,dy\,dz}{PQ}=M\sum_{0}^{\infty}\frac{3}{2i+3}\frac{\left(a^2\frac{d^2}{df}+b^2\frac{d^2}{dg}+c^2\frac{d^2}{dh}\right)^i}{2i+1!}\frac{1}{\sqrt{f^2+g^2+h^2}},\quad\ldots(4)$$

where M is the mass of the ellipsoid. Equation (4) gives the harmonic expansion of a solid ellipsoid of unit density; and it will be observed that, in that form, it gives a very simple proof of Maclaurin's theorem, since the operator

$$a^2\frac{d^2}{df}+b^2\frac{d^2}{dg}+c^2\frac{d^2}{dh}$$

is unaffected by the addition of the terms

$$k\left(\frac{d^2}{df}+\frac{d^2}{dg}+\frac{d^2}{dh}\right).$$

By means of (4) it is easy to find what Thomson and Tait have called the exhaustion of potential energy, due to the mutual action of two solid ellipsoids, and then by spherical harmonic analysis, to determine approximately the nature of the forces acting between them.

Pursuing the same method, we next find that the potential at an outside point (f, g, h) due to an ellipse of density 1, with its axes $2a$, $2b$, in the axes of co-ordinates x, y, is

$$2\pi ab\sum_{0}^{\infty}\frac{\left(a^2\frac{d^2}{df}+b^2\frac{d^2}{dg}\right)^i}{2^{2i+1}\,i!\,i_{+1}!}\frac{1}{\sqrt{f^2+g^2+h^2}}\quad\ldots\ldots\ldots\ldots(5).$$

From (5) it is easy to pass to the potential of a magnetic pole at f, g, h, due to unit current circulating in the boundary of the ellipse, and thence to the expansion in harmonics for the mutual potential energy of the currents in two elliptic circuits in any positions.

Results similar to (4) and (5) can also be easily found for solid parallelopipeds and for rectangular circuits.

The series here found cease to be convergent within certain regions including the origin. The expansion of the potential in those regions must be found by an independent investigation.

It may be remarked that the harmonics in the form of differential coefficients, given in (3), (4), and (5), may easily be expressed in terms of the corresponding surface harmonics, according to the following theorem :—

Let r be the radius vector from the origin O to any point P, and let $f\left(\frac{d}{dx},\frac{d}{dy},\frac{d}{dz}\right)$ be any homogeneous operator of the ith degree operating upon the reciprocal of r, then

$$f\left(\frac{d}{dx},\frac{d}{dy},\frac{d}{dz}\right)\frac{1}{r}=(-1)^i\frac{1}{r^{i+1}}f\left(\frac{d}{dx},\frac{d}{dy},\frac{d}{dz}\right)(Q_ir^i),$$

where Q_i is the biaxal harmonic of the ith degree, whose pole is in OP, and which may therefore be expressed in the usual way, according to Laplace's addition formula. The differentiations are then easily performed.

III. "On the Origin of the Parallel Roads of Lochaber, and their bearing on other Phenomena of the Glacial Period." By JOSEPH PRESTWICH, M.A., F.R.S., F.G.S., &c., Professor of Geology in the University of Oxford. Received March 27, 1879.

(Abstract.)

Of the various hypotheses that have been brought forward since the time of Macculloch and Dick-Lauder to account for the origin of the Parallel Roads of Glen Roy, the one so ably propounded by Mr. Jamieson, in 1863, has been most generally received and adopted. It is a modification of the views originally expressed by Agassiz, to the effect that the barriers of the lakes,—to the shore action of which both the above-named geologists attributed the "roads," but were at a loss to account both for the formation and removal of barriers,—had been formed during the glacial period by glaciers issuing from Glen Treig and Glen Arkaig, supplemented by others from Ben Nevis. The subsequent determination, by the Scotch geologists, of an intermediate milder period succeeded by a second cold period, led Mr. Jamieson, with whom the preglacial and glacial deposits of Scotland had been a subject of especial investigation, to conclude that the extension of these two glaciers took place during the second cold period, which he thinks was of little less intensity than the first, and that, while the glacier from Glen Arkaig blocked up Glen Gluoy, the glacier from Glen Treig formed a barrier to Glen Roy. He observes, "Grant, then, these two ice-streams, one in the Great Caledonian Valley and the other at Glen Treig, and the problem of the Parallel Roads can be solved, provided we allow that glaciers have the power to dam such deep bodies of water as must have occupied Glen Gluoy and Glen Roy."

Mr. Jamieson, in support of this view, adduces the extensive glaciation apparent at the entrance both of Glen Arkaig and of Glen Treig, and shows that near the entrance of Glen Gluoy there are ice striæ, pointing W. 5° N., or in the direction that a glacier coming from Glen Arkaig would take, and that, in the Spean Valley, opposite Glen Treig, the ice striæ are transverse to the valley, or in the direction of the axis of Loch Treig, while on either side they point respectively up and down Glen Spean. He infers, consequently, that the central

portion of the glacier ascended the opposite hill to the Col of Glen Glaster, while another portion passed down the valley blocking Glen Roy, and a third portion travelled up the valley eastward to the Pass of Makoul, and thence into the valley of the Spey.

The "roads" were, he considers, formed by long-continued shore action at each successive level of the lake, that level being determined by the height of the cols over which the lake waters escaped. In proof of the long duration of the lakes, Mr. Jamieson refers to the dimensions of the "roads," and the large size of the mounds at the junction of Glen Turret and Glen Roy, and of that at the entrance of the Gulban in Glen Spean, which mounds he considers to be deltas formed by the respective streams flowing into the old lakes.

To these views it has been objected, by Mr. Milne-Home and others, that it is difficult to conceive the glens to the north of the Spean Valley to have been filled with water while at the same time those on the south were filled with ice, and he advocates a detrital barrier formed of clay, sand, and gravel, by marine origin, when the sea stood some 3,000 feet higher than at present. There is, however, a want of evidence of marine action, and it is difficult to imagine that beds of loose clay, sand, and gravel, formed in this way, could be water-tight, while there is a further difficulty in accounting for the origin of the materials and the subsequent removal of such vast barriers.

Professor Nicol, also, has pointed out that, had lakes existed in Glen Gluoy, Glen Roy, and Glen Spean for the length of time required to form the "roads" by erosion, and to accumulate the deltas, the cols by which their surplus waters escaped during those periods must have cut a channel in the rocks in the same way that the rivers (which now represent the same drainage, or probably less) have since excavated their channels in the present valleys; whereas, although there are indications of water-wear in the passes, nothing like a defined river channel exists. Professor Nicol has attempted to explain the facts on the theory of the "roads" being sea beaches. But the absence of corresponding beaches outside those glens—the limitation of the highest "road" to Glen Gluoy—and of the second and third to Glen Roy, and the want of corroboration by marine remains in any of the various drift beds, renders the marine hypothesis inadmissible.

Sir John Lubbock, looking at the form of the "roads" which has been described by Macculloch as parallel layers applied in succession to the sides of the hills, contends that such a form is incompatible either with the heaping up of materials on a shore line, or with their removal by erosion, as in the one case a notch and in the other a projecting ledge in the hill side would be formed, whereas, with one exception of a superior talus pointed out by Macculloch, no such structure exists. Sir John points out that a parallelism between the slopes may, however, have been formed by wavelet action, in con-

sequence of the detrital matter taking, as it successively fell and was removed, the same angle of repose as that which the detrital slopes originally had, that angle being the same in water as in air. But the angle of the slopes varies from 25° to 35°, whereas, the angle of repose of such loose materials as would fall down the slopes is as high as 38° to 40°.

Besides these objections to Mr. Jamieson's hypothesis, which the author considers valid, he points out the difficulty of conceiving that the Arkaig glacier, which was fed by mountains not more than 2,000 or 3,240 feet high, and had to travel, not down a steep gradient, but along a level lake basin 12 miles long, then to traverse Loch Lochy, 457 feet deep, could have ascended the hills at the entrance of Glen Gluoy to a height of not less than 1,200 feet, while at the same time a pass existed at the head of the glen only 500 feet high, which presented a ready outlet to the west coast. Or, if prevented from taking that course, why should not the Arkaig glacier, when it issued into the Great Glen, have taken the line of least resistance and passed either up or down the Great Glen, where the gradients are so small?

It is a question also whether active glaciers such as Mr. Jamieson requires, could have formed permanent dams to the large bodies of water pent up in Glen Gluoy and Glen Roy. Glacier lakes are occasionally formed in the Alps, as in the instance of the Margelen See, by glaciers when below the snow line, passing lateral glens whose streams they dam back, but they are much more common in the Himalayas. These lakes, however, never last many seasons. The glacier is constantly on the move, and so long as it presents an unbroken front to the lake, so long is the barrier efficient, but when in the progress of the glacier a fissured mass of ice comes forward, the water at once escapes with greater or lesser rapidity, and cannot again accumulate until the defective ice has travelled past or the leak is repaired by winter frosts.

In the known recent instances of such glacier lakes, they are so small compared to the great mass of the glaciers, that the latter are not affected by the escape of the lakes—the fissures only are enlarged; but with the great size of the Glen Roy and Glen Spean lakes, and the enormous pressure exercised by columns of water 500 to 800 feet deep, would the preservation of the barriers and the restoration of the lake be possible?

Equally difficult is it to imagine the existence of such vast glaciers as those of Glen Arkaig and Glen Treig, while the opposite glens of the Gluoy and Roy remained free from ice. For the hills around Glen Arkaig are but little higher than those around Glen Gluoy, and not so high as those around Glen Roy; those to the eastward of the latter valley are in fact nearly equal in height to the range on the south of the Spean with the single exception of Ben Nevis and the

adjacent Ben Aonach. The difference is too slight to allow of so great a variation in the level of the snow line, and the cause suggested by Mr. Jamieson, viz., a great difference in the rainfall such as it now obtains in this district, can scarcely be maintained, for although the annual fall at Fort William is 86 inches, and at Laggan 46 inches, the rainfall at Roy Bridge has now been found to be as much as 62 inches. Further, for the argument to be of any value, it should be shown that in the country further eastward where the rainfall is much less, there was the like absence of glaciers during the second period, whereas Chambers and other geologists, including Mr. Jamieson himself, have shown that during that period local glaciers descended from every mountain range, approaching or exceeding 3,000 feet in height—a height attained as already remarked by the hills to the north as well as by those to the south of the Spean.

With respect to the so-called "deltas" of the Turret in Glen Roy, and of the Gulban in Glen Spean, which are supposed to have accumulated during the long time that the lakes filled the valleys, the author shows that the structure of the former is not in accordance with the bedded structure of deltas, but on the contrary that it is formed of unstratified moraine *débris* 50 to 80 feet thick, with a thin coating of gravel water-worn and reconstructed from the underlying mass, and that the angle of terminal slope is not that of original deposition, but is due to wearing back of the terrace by the Roy, and the fall of the *débris* by weathering.

The foregoing are the objections raised by the author to the hypothesis of Mr. Jamieson, but while objecting to this exposition of the glacial theory, he considers that that theory affords the most satisfactory solution of the problem, only that he would suggest a different interpretation in explanation of the phenomena, the description of which he does not think it necessary to repeat.

Views of the Author.

Dismissing the hypothesis of local glaciers of the second period of glaciation, the author falls back upon the original idea of Agassiz with the development acquired by more recent research, and assigns the Lochaber lakes to the close of the first period of great glaciation. He considers the phenomena are due to the peculiar physiographical conditions of the district, and shows that owing to the configuration of the country, the drainage of the Ben Nevis range instead of flowing off from the centre to two or more sides, is all diverted northward into the lower part of the Spean Valley and the Great Glen near Fort William. These conditions which now give this area an excess of water drainage, must in the like manner, during the glacial period, have there led to an exceptional accumulation of ice.

The observations of MacLaren, Chambers, Milne-Home, Jamieson, James Geikie and others, sufficiently prove the great thickness of the ice covering in this part of Scotland during the first period of intense glaciation. On the flanks of the Ben Nevis range, glacial striæ extend to a height of more than 2,000 feet, while on the hills facing this range, and on the opposite side of the Spean Valley, such striæ have been traced to nearly the same height, as for example to nearly the summit of Craig Dhu near the entrance of Glen Roy, and on the hills above the Rough Burn by Mr. Milne-Home to a height of 1,600 feet. Everywhere, the rocks in the lower parts of Glen Spean are intensely glaciated, as are also those at the head of Glen Roy and around the Cols of Glen Glaster and Makoul. At the same time, the erratic blocks with those beds of sand and gravel of foreign origin which have been found widely distributed over the hills around Glen Roy to heights of from 800 to 2,100 feet, afford confirmatory testimony of the depth of the land ice which then covered the country. There can be little doubt therefore of the vast extent and general spread of the ice-sheet at the first period of glaciation.

With the incoming of this glacial period, local glaciers must have descended from every mountain range, and so long as the glacier of one steep glen became confluent with another of the same chain flowing in the same general direction, so long would their course be uninterrupted, and the propelling and abrading force maintained, as in the Alps at the present day; but when, emerging from these glens into valleys of small gradients dividing the several mountain chains, they met with glaciers descending from these other ranges, their progress was not only subject to be checked, and their forces neutralised, but their course diverted, for if the lines of natural drainage were barred, the ice took those of least resistance, although such might be up-hill and against the lines of drainage. This, however, could not be effected without excessive pressure and heaping up of the ice at the points of junction.

These interferences must have been especially frequent in the valley of the Spean. On the one side, the glaciers descending the steep ravines of Larig Leachach, the Cour and others on the northern flank of the Ben Nevis range, issued into Glen Spean and projected across it to the Glen Roy hills opposite. Below to the west, the great Nevis Glen glacier emerged into the valley of the Lochy, while above to the east the great glacier from Glen Treig turned down Glen Spean, but, meeting with the aforesaid group of glaciers from Ben Nevis, was partly diverted over the flanks of Craig Dhu, and upon the entrance to Glen Roy.

While the glaciers from this system of mountains were becoming confluent in and filling Glen Spean, those from the opposite range of hills were descending Glen Roy, Glen Feitheil, the Rough

Burn, and the other ravines of that chain, and coming into collision with those of the Ben Nevis range. In the same way, the valley of Loch Eil, Glen Nevis, Glen Mhuilinn, Glen Loy, and others were focussing their glaciers upon the end of the Great Glen north of Ben Nevis, barring in that direction the passage of the ice down Glen Spean, and diverting it northward towards Loch Lochy and Loch Oich.

The ice therefore descending Glen Spean, in consequence of these obstructions, accumulated in mass in the lower part of that valley opposite Glen Roy, until overcoming further resistance and confluent with the Ben Nevis glaciers it wheeled round into the Great Glen at Loch Lochy, where the united stream found not only a more contracted passage, but met also at right angles the glacier issuing from Glen Arkaig, by which it was forced against and up the entrance to Glen Gluoy opposite.

This direction of the flow is in accordance with ice striæ noticed by Mr. Jamieson on the flank of Strone-y-Vaa, and by Mr. J. Campbell, of Islay, to the west of Spean Bridge; but it is not so readily reconcileable with those at the entrance of Glen Gluoy. It may be a question, however, whether the striæ, in the Spean Valley, facing and flanking the entrance to Glen Treig, are all to be interpreted in the way suggested by Mr. Jamieson. There is no doubt from the direction of the striæ, and the position of the transported boulders, that the mass of the Treig glacier struck across the valley of the Spean, and turned down its channel westward; but that a part ascended to the Col of Glen Glaster, and another passed up the Spean Valley is doubtful. It is more probable that this glacier after traversing Strath Spean, met with glaciers coming down Glen Feitheil and the Rough Burn, which caused it to turn down the Spean Valley, while it barred the passage of these other glaciers and diverted them up the valley in the direction of Loch Laggan and over the Pass of Makoul. The direction of the striæ observed by the author between the Rough Burn and Moy, points, he considers, to ice coming down from the hills on the north and joining the main east stream.

The effects of these great conflicting ice streams were not confined to the piling up and accumulation of the ice only. Although glaciers confined by the walls of narrow glens, and descending steep slopes exercise great abrading power, the observations of Charpentier and others show that when they emerge into broader and flatter valleys, they may pass over beds of loose detritus without disturbance except that of pressure. The terminal moraines of the many glaciers emerging into Glen Spean may, according to the varying conditions of the ice, have been pushed forward or rolled over by the ice, while the meeting of conflicting glaciers must have led to the deposition and heaping up of the glacial *débris* at the points of junction. The many checks and

blocks that must have occurred during the growth of the great ice-sheet—the neutralisation of the ice-force in one place, and the centralisation of it in others—will serve to explain much that is peculiar in the distribution of this sub-glacial *débris* or Till, not only in Lochaber, but in other parts of the country, and at all levels.

The author then points out the many mounds and terraces in the Spean Valley formed of moraine detritus, though since levelled and often masked by a covering of gravel due to subsequent water action. To this cause also he attributes the large accumulation of *débris* at the entrance to Glen Roy, between Bohuntine and Glen Glaster, where he shows it to be in places 200 to 300 feet deep, and to consist of a light grey argillaceous unstratified matrix with angular fragments of the local rocks capped by stratified gravel. The mass rises nearly to the level of the lower parallel road. Mr. Milne-Home has pointed out a similar deposit at the entrance to Glen Collarig, while the large mass at the entrance of Glen Spean, and now forming Unachan Hill, rising to a height of 613 feet, has been often described. The importance of these great deposits in the position occupied by the main ice barriers cannot be overlooked, for though river action and denudation has removed much of them, the portions which remain show how materially they may have contributed with the ice in forming the great lake barriers.

The next question discussed is the height of the land in relation to the sea at the period of the great glaciation, as it is not possible to suppose that with the great changes of level which took place subsequently, there is now a return to the *status quo ante* of the earlier period; and the author sees reason to conclude that the land then stood at not less than from 1,000 to 1,500 feet higher than at present, so that the Irish Channel was then above the sea level, and land extended a considerable distance westward from the present coast of Scotland.

This was followed by a submergence of not less than 1,200 to 1,500 feet in central and northern England, Wales, and Ireland, and of 600 feet in the southern part of Scotland, as proved by the occurrence of marine shells at those heights. There is no similar evidence, however, to show that it was more than 300 feet in Aberdeenshire, or 175 feet off the north-west coast. Still, it is not to be concluded on negative evidence alone of the absence of marine shells that the sea reached no higher over the north of Scotland. Assuming there at all events a submergence of 400 to 500 feet below the present level, this, added to the previous elevation of 1,000 to 1,500 feet, would establish a difference of 1,500 to 2,000 feet between the period of great glaciation and the succeeding period of submergence.

This difference of level would produce a twofold effect upon the climate—the one resulting from altitude which would be equal to a rise in the mean temperature of from 4° to 6° F., and that caused by

the conversion of a continental area into an archipelago, for then in consequence of the submergence, the Highlands of Scotland became surrounded by an open sea with only the higher peaks of England, Wales, and Ireland above the waters, and the warm currents of the Atlantic reached the foot of the Scottish mountains. The effects of the two causes could not be less than from 12° to 15° F., which is about equivalent to the difference of climate between Paris and St. Petersburg. There is also to be taken into account the probable increase of heat consequent on the gradual diminution of those conditions, whatever they were, to which was owing the cold of the glacial period.

The effect of those changes was to produce a thaw which gradually led to the destruction of the great ice-sheet, though subsequently other changes brought for a time a return of cold sufficient to maintain local glaciers in the higher mountain ranges. The conditions under which the ice-sheet would be placed during this period would be similar to that of glaciers where they extend below the snow line. In Europe the glacier gradients are usually so steep and the ice is so fissured, that even in such positions water rarely lodges on the surface; still it does at some places give rise to small pools and streams, but in the Himalayas, where glaciers descend into wide valleys with small gradients, and the summer heat is considerable, the glaciers often become covered with tarns and small lakes. They have been noticed by Sir J. D. Hooker and more lately by Colonel Godwin-Austen, who describes in the middle of one of these glaciers, a series of such lakes, some being 500 yards in length and 200 to 300 in breadth, and of great depth. As intervening barriers give way, these lakes descend in course of time from lower to lower levels, and finally escape.

In the same way the old ice-sheet must have become covered with pools and lakes, for owing to the irregular surface of the ice, and the inevitable absence of all channels of drainage, the water must everywhere have lodged, until channels were formed, and a means of escape established. The extent of these bodies of water would depend upon the height and permanence of the obstructions. In the Lochaber district they were, owing to the causes before named, of great size and permanence, such as to form high barriers at the entrance to Glen Roy, Glen Spean, and Glen Gluoy, behind which the waters accumulated and rose until they found a channel of escape over the cols at lower levels, when a permanent water-level would be established so long as the main barriers existed.

It is well known that the Parallel Roads are terraces composed of perfectly angular fragments of the local rocks with a few rounded pebbles both local and foreign to the district. The former show an entire absence of any prolonged beach wear. The wear of the latter is due to other causes. There is also an entire absence of any notch

or cliff line such as would be due to the wearing back of a shore line, and of any projecting ledge such as would result from the throwing forward of the shore *débris*. The slope of the hills above and below the "roads" varies from 25° to 40°, and the inclination with the horizon of the "roads" themselves, which are from 50 to 70 feet wide, varies within the limits of from 5° to 30°.

Of the internal structure of the "roads" very little is known. The only published section is the one given by the Rev. Thomas Brown in his paper on the "Parallel Roads." This shows a thick substratum of about 20 feet of sandy clay with angular *débris* and boulders, covered by a thick seam of stratified clay, and then by 2 to 3 feet of clay with stones. There is no appearance in this section nor in another unpublished section of Mr. Brown's, of any such structure as would result from successive additions to the ledge by the tipping over of *débris* removed from the shore.

Although therefore the "roads" indicate a line of water-level, there is nothing in their form or structure to show that they have been formed by the long-continued action of lake waters on a shore line. To what then are they to be ascribed?

The first or highest "road" is confined to Glen Gluoy, the second and third to Glen Roy, and the fourth or lowest to Glen Roy and Glen Spean. What the conditions were immediately antecedent to the formation of the first, second, and fourth road, is not shown, but in the case of the third road, the conditions preceding its formation are to be traced uninterruptedly from the conclusion of No. 2 "Road." When the lake stood at the level of "Road" No. 2, its waters escaped by the col leading to Glen Spey, while when they stood at the level of No. 3 "Road," they escaped by the Glen Glaster Col. Now as there is a difference of 76 feet between the height of the two cols, it is evident that a barrier must have existed on the latter col during the time the lake stood at the higher level. Whether the barrier was detrital or ice-formed is immaterial for the argument. In both cases, either by gradual weathering or melting, the time would come when the barrier would be lowered in some place to the level of the lake.

Now, it is well known to engineers that a breach once established in a detrital barrier becomes so rapidly enlarged that, if not at once stopped, nothing can stay the rapid destruction of the barrier, as, in the case of the Holmfirth, Crinan, and other floods. Nor is evidence wanting of similar catastrophes in connexion with glacier lakes. In the notable case of the Gietroz Glacier barring the valley of the Drance, a lake was formed which attained a length of nearly 2 miles, and a depth at the barred end of 200 feet. So rapid was the discharge when the barrier yielded that the lake was drained in twenty minutes. The still greater flood recorded by Vigne, which descended a branch of the Indus in one day for a distance of 125 miles, has since been shown by

Mr. Drew to have been caused by the bursting of a detrital barrier formed by a landslip. In consequence of this a lake had been formed, which he estimates to have been 35 miles long by 1 mile broad and 300 feet deep at its lower end. The whole was drained in a day.

In the same way, it is to be assumed that the Glen Glaster barrier, which was probably formed by a remnant of the glaciers descending from the mountain ranges (2,994 feet) at the head of the glen, at last gave way with great suddenness, and caused the rapid fall of the waters from the level of the higher "road" in Glen Roy to that of that glen's second "road," at the height of the Glen Glaster Col, when the escape of the waters was stopped.

Now, it must be borne in mind that, at this time, the great mantle of snow and ice which had so long covered the country, was passing away, leaving the surface of the hills in Glen Roy covered with a thick coating of angular local *débris* mixed with sand and clay, the result of the intense cold and of the decomposition of the underlying schistose and granitic rocks. This and the glacial *débris* must have long remained bare and unprotected by vegetation; at all events, that below the water-line was so. Now, the angle of repose of purely angular and subangular *débris* varies within the limits of from 35° to 48°, but that of clayey sands, which when dry is from 21° to 37°, becomes, when saturated with water, as low as 14° to 22°. The angle of repose of the hill-side *débris* would, therefore, depend on the relative proportion of the angular materials and their matrix, and on the extent of saturation. The slopes of the hills being on the whole greater than that of the angle of repose of the saturated under-water rubble, this latter, easily set in motion on to the settlement of its constituent parts as the water drained from it, would as the level of the lake water fell, tend to slip or slide down with the falling water, and this slide would continue until the disturbing cause ceased, and the momentum of the mass was checked by the inertia of the water* gradually coming to rest on reaching the level of the col of escape. The effect of the arrested slide, combined with the state of maximum saturation of the mass, would be to project it more horizontally forward, and form a ledge. This ledge, modified slightly by subsequent subaërial action and weathering, and by the dressing of its slope on the occasion of the next fall of the lake, constitutes the "road."

As the results are alike in the case of the other "roads," although the causes which led to them must have been the same, there is not the same evidence of a minor col-barrier; and it is shown that there is nothing incompatible in the features of the ground with the existence of such barriers, or rather that there is some evidence in each glen, however slight, of water lines at levels higher than the "roads." There are difficulties in the way of the lower

* Even now after heavy rains large slides take place on the steeper slopes.

"road," No. 4, which extends through Glen Roy and Glen Spean, that need discussion, but they are not considered more serious than these which attend the other hypothesis.

The theoretical inferences of structure agree fairly well with the facts so far as they are known. Mr. Brown's "clay with boulders indistinctly stratified with thin (lenticular) layers of sand," represents the sliding detrital mass,—the finely stratified sand and clay; the sediment which subsided from the muddy lake waters after their fall; and the 2 to 3 feet of stones with clay,—the subaërial fall of *débris* from the slopes above. In the substratum and overlying sediment, Mr. Brown found four species of fresh-water diatoms, while he found none in the upper bed. This fact serves to confirm the subaqueous origin of the body of the ledge, while it tends further to disprove the marine hypothesis.

It has been before observed that, in all the cols, there is an entire absence of a defined water channel, such as would be worn by the long-continued flow of a river, but in all of them there are traces of strong water action, such as might result from the temporary passage of a large and rapid body of water.

With respect to the main barriers acting as dams to Glen Gluoy, Glen Roy, and Glen Spean, they were due, as already pointed out, to the circumstance of an accumulation of ice at these spots so excessive and so high as to last long after the ice generally in the lower tracks had given way. Not, however, that any ice barrier could have been permanent for a great period of time, but this the author's hypothesis does not require. In any case, an ice-barrier in a state of rest will form a more effective barrier than when in motion.

Passing over the barrier at the entrance to Glen Gluoy, it is shown that the point where the Glen Roy barrier existed is that where a glacier coming down Glen Roy would meet in opposition the ice from Ben Nevis and the Spean Valley; and that this glen was occupied by a glacier is proved by the occurrence of glacial striæ on the rocks forming the bed of the valley near Dalriach, and of Till, or boulder clay, lower down the valley, nearer Achavady. But the great mass of the latter lies precisely on the spot where the Ordnance Survey have placed the line of barrier; it was there heaped up by the same conflicting causes that produced the barrier of ice. That it was originally larger and higher is proved by its occurring on the two sides of the valley, the river having worn a channel through it, and by the presence upon it of thick beds of water-worn and water-strewn gravel, the materials of which have been derived from the underlying deposit, and which was formed, in all probability, by the rush of the waters on the bursting of the barrier, for, in many places, the gravel is thrown back and over, as though by downward and outward pressure of water in motion. This detrital mass extends for a length of 2 miles

or more, and attains at one point where the carriage road crosses it, a height of 752 feet, that of the lower Parallel Road just above it being 860 feet.

The great barrier needed at the entrance of Glen Spean presents more difficulty on account of the width and depth of the valley. The river is here 200 feet above the sea level, and the length of barrier required, 4 miles in extent. It is, however, precisely on this ground that the great glaciers of Ben Nevis met the ice stream coming down Strathspean. The ground rises, also, rather rapidly on both sides of the river, and from behind the line of barrier marked by the Ordnance Survey the rise is continuous to the summit of Unachan Hill, 613 feet high. Now, this hill and the rising ground on the flanks of the valley consist of a thick substratum of till or boulder clay, with a covering of gravel, the latter formed in greater part, if not entirely, from the destruction of the former, so that there is little doubt that the detrital barrier here also was at one time much more important. Still, although the detrital matter formed a considerable element, the author believes that the great mass of ice constituted the essential element in the barriers.

The Till, although accumulated in larger masses in the above-named sites, is found in places all up the valley, generally in the form of terraces covered by gravel, as at Inverroy, Murlaggan, Inverlaire, and elsewhere. There is one feature common throughout, namely, the levelling and terracing of this glacial *débris* by subsequent water-action, which could not have been effected in the still waters of a lake bed, but probably took place on the bursting of the main barriers and during the rapid outflow of the waters. To this cause may, also, be owing the terraces of the Turret, Gulban, and others, for it is to be remembered that the same glens which subsequently furnished the streams, had previously furnished glaciers. Though these happened to be at the mouth of streams to which their origin has been ascribed, similar terraces occur elsewhere where there are no streams, and where the structure is the same. The levelling of the original glacial mounds having been effected at the time of the drainage of the lakes, and having been then covered and masked by gravel, the terminal slopes were either formed at the same time by the outpouring waters as they fell to a lower level, or subsequently by wearing back by the present streams. Some of the terraces ascribed by Chambers to sea margins may be due to these causes, at the same time it is possible that some of his lower terraces outside the Spean Valley, at levels of 70 to 200 feet, may be due to later marine action, and correspond with the raised beaches of Jura and the coast further north, at levels extending from 40 to 160 feet or more.

An objection may occur to the hypothesis here suggested for the formation of the Parallel Roads, in that, with elements so

variable as the problem has to deal with, the parallelism of the "roads" with the horizon, which has been remarked on by all observers from Macculloch downwards, could not have been maintained. For the detritus of the hill sides varies, however slightly, in the relative proportions of rock fragments and soil, while the slopes above and below the "roads" also vary in their angle, so that, as these conditions varied, so would the momentum of the sliding mass vary, whilst the resisting force of inertia would remain the same. The consequence would necessarily be, that the slide would continue at some places to a lower level than at others, and the line of the "roads" could not be throughout parallel with the horizon.

There is no doubt that, to all appearances, the "roads" are perfectly level, and such was the opinion of the author when standing on them and while working with the excellent 1-inch map of the Survey; but afterwards, on referring to the elaborate 6-inch maps, he found the levels there given clearly show that the "roads" are neither at the exact height of the cols nor are they perfectly parallel with the horizon, after allowing for the variable inclination of the "roads," and for the observations being made in their centre.

Instead of a perfect level water line, the "roads" are really slightly waved, the difference between the highest and lowest point being in the four "roads" taken in descending order, 15, 11, 15, and 12 feet; and while the level of the higher "roads" is in most instances *below* those of the several cols of escape, that of the lower "road" is in all instances *above* it. Thus in Glen Gluoy, the "road" which is never more than 1 foot above the level of the Turret Col, is in places 14 feet below it. The Glen Roy "roads," Nos. 2 and 3, rise 2 and 4 feet above the col of the Spey, and sink 7 and 13 feet below it, whereas No. 4 "Road" is never less either in Glen Roy or Glen Spean than 2 feet, and rises at places to 10 and 14 feet, above the Pass of Makoul. Further, as the curves formed differ for each "road," the variations cannot be due to a common cause, such as subsequent movements of the ground, but must be owing to differing conditions in each case local. Nor do the levels on the two sides of the valleys correspond; they often vary as much as 7 or 8 feet. It is therefore not possible to reconcile these variations with the hypothesis of the "roads" being lines of water level due to shore action; nor is the very variable inclination of the "roads" themselves compatible with that view, for on a strand where the slopes do not differ more than in these glens, there could not be that irregularity of surface which exists in the "roads."

In the instances of the higher "roads," the lower level of the curve is possibly due to the steeper slopes, whilst the fact that the lower "road," No. 4, remains above the level of the Pass of Makoul, may be due to the circumstance either of the slopes being less, or more probably to the fact of the lake having been so very much larger, the

escape of the waters was consequently more prolonged, and the fall slower.

Various phenomena in connexion with the great ice-sheet in Lochaber and their connexion with the general question, are next considered. The author objects to the term of *moraine profonde* to describe the drift of sub-glacial origin, as apt to lead to misunderstanding, although Hogard and other geologists have used it in the wider sense. It is evident that the old ice-sheet acted under very different conditions to an ordinary glacier, and it is better to use such terms as moraine detritus or sub-glacial detritus for the sum total of sub-glacial products of the former, than employ a term which was originally and still is generally restricted to a single and comparatively small product of the former.

Besides this sub-glacial *débris*, there is the large quantity of *débris* that must have been scattered over the surface of the ice during its melting by streams and rivulets, or spread out in the temporary lakes which were formed at all levels, and may have given rise in many instances to sand and gravel terraces of variable extent. But though true beaches are formed in certain glacial lakes, for ledges or shelves, such as constitute the Parallel Roads to be formed, a number of conditions must have concurred—such as sufficient slopes, a detrital covering, barriers at the mouth of the glens, and cols of escape at their upper end.

As the barrier ridges on the old ice-sheet melted, or burst, the waters escaped to lower levels, carrying with them on or beneath the ice, a large portion of the surface detritus. Formed at all levels up to 2,000 feet or more, these glacial lake waters in descending to lower levels met with other large bodies of water, and the transporting forces increased in power till the last stage was reached and open channels formed in the distant plains, leaving as marks of their passage down the valleys—here great banks of gravel—there deep beds of sand, according to the distance from the point of outburst. To these floods, combined with river inundations and the modifications since brought about by fluviatile action, are due the various forms of escars, terraces, and other less defined detrital accumulations.

Conclusions.

The general conclusions drawn by the author from the phenomena in Lochaber and surrounding district, are :—

1st. That at the period of the first great glaciation of Scotland, the ice-sheet in Lochaber attained a thickness of not less than 2,000 to 2,500 feet, but that in consequence of the peculiar physiographical conditions of the district, the large ice-currents from Ben Nevis so clashed with others in the Spean and Lochy Valleys, that a block

ensued which led to an exceptional heaping up and accumulation of the ice in front of Glen Roy and Glen Gluoy.

2nd. In consequence of the lowering and partial submergence of the land and its conversion from a continental area to an archipelago, combined possibly with some other more general cause, an amelioration of the climate took place attended by a gradual melting of the ice-sheet in all the lower tracts. The snow and ice wasted from the valleys and from the lower mountain summits, and in the absence of any established water-courses, the hollows and depressions in the ice, when not fissured, were converted into pools and tarns, until the continued liquefaction opened out surface channels or interior fissures, by which the water could ultimately escape.

3rd. That pending the establishment of natural lines of drainage, and in presence at places of unusual obstruction, the water accumulated in some valleys in larger bodies or lakes; and if in these cases, the mouth of the valleys being closed by the main ice-barriers, other channels of escape such as cols or passes communicating with adjacent glens or valleys presented themselves, the water overflowed through these channels as soon as it rose to the height of such cols or passes. Or should the cols have been also barred by ice, that ice would have given way as soon as the increasing height and pressure of the water proved sufficient. When this happened, the water would at once fall to the fixed level regulated by the col, and thus no record, such as we have in Lochaber, might be left of the presence of the original bodies of water.

4th. In the Lochaber district, while the exceptional accumulation of ice in the Spean Valley barred the entrance of the glens on the north side of that valley, their passes were also blocked by remnants of the great ice-sheet; and the formation of the detrital shelves is due to the sudden bursting of these minor barriers, when the waters of the lake were discharged with great rapidity, until they fell to the level of the col. Under these circumstances, the mass of loose *débris* covering the hill sides below the line of water level, gave way and slid after the retreating waters, until stayed with greater or lesser abruptness, according to the angle of slope and the volume of the mass, on the discharge ceasing and the waters coming to rest. The shelves so formed, modified slightly by subsequent subaërial action, constitute the "roads."

5th. The moraine detritus in places where the glaciers clashed, and where their progress consequently became checked or delayed, tended also to accumulate or heap up, and in this way in the Lochaber glens added to the strength and permanence of the ice-barriers.

6th. While the moraine detritus was irregularly distributed under the ice, or massed in particular places, the *débris* projected on the surface of the ice-stream and contained in its body, was either left *in*

situ on its liquefaction, or else was, as the result of the great floods consequent upon the bursting of lake barriers, carried successively to lower levels, leaving here and there banks of sand and gravel at various heights on the hill sides. These destructive floods combined with the incessant river inundations due to the same general thaw of the great ice-sheet, carried down and spread out in the valleys and plains the great beds of gravel and sand, which, with the modifications since brought about by long continued fluviatile action, have given rise to various forms of escars, terraces, and other less defined accumulations of these detrital materials.

May 8, 1879.

THE PRESIDENT (followed by Lord LINDSAY, Vice-President) in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Sensitive State of Electrical Discharges through Rarefied Gases." By WILLIAM SPOTTISWOODE, P.R.S., and S. FLETCHER MOULTON, late Fellow of Christ's College, Cambridge. Received April 2, 1879.

(Abstract.)

It has frequently been remarked that the luminous column produced by electric discharges in vacuum tubes sometimes displays great sensitiveness on the approach of the finger, or other conductor, to the tube. This is notably the case when with an induction coil a very rapid break is used, or when with any constant source of electricity an air-spark is interposed in the circuit leading to the tube. The striking character of the phenomena, and the opportunity which they showed for affecting the discharge from the outside during its passage, led the authors of this paper to consider that a special examination of this sensitive state would be desirable.

All the circumstances under which sensitiveness is produced appear to agree in requiring, first, that there should be a rapid intermittence in the current leading to the tube; and secondly, that the individual intermittent discharges should be small in quantity and extremely brief, if not instantaneous, in duration. Both these requirements are fulfilled by the methods used in the present investigation, viz., a

Holtz machine with a suitable air-spark between the machine and the tube, and a small coil with a rapid break.

If a conductor be made to approach a tube conveying a sensitive discharge, due to an air-spark in the positive branch of the circuit, a series of effects is produced, of which the feeblest and the strongest are the most pronounced. The transition from one to the other is so rapid that the intermediate phases may be easily overlooked. In the first case, the luminous column is repelled by the conductor; in the second it is broken into two parts which stretch out in two tongues towards the point on the tube nearest the conductor, while a negative halo appears between them.

That these effects are due to the inductive action of the conductor, or more particularly to re-distributions of electricity in it, co-periodic with the air-spark, and not to any permanent charge, is shown by the following experiments. A non-conductor, whether charged or not, is without effect. The effect of a conductor increases with its size or capacity, and with its proximity to the tube, until the fullest effect (viz., that given by an earth connexion) is produced. That the effects are not due to electro-dynamic, or to magnetic action, is shown by the fact that a coil of wire produces the same result, whether the ends be joined or not. The effects of an iron core and helix with open ends are often comparable with, and sometimes equal to, those when the ends, being connected with a battery, the whole becomes an electro-magnet. The effect upon the interior is, in fact, due to the relief given by the conductor to the electric tension on the outer surface of the tube and the space around it, caused by the individual discharges.

Instead, however, of connecting a point on the tube with a large conductor or with earth, we may connect it with one or other terminal of the tube. And a further study of the subject shows that all the phenomena due to action from without may be produced by means of one or other of these connexions. Connexion with the non-air-spark terminal gives the relief effects described above; connexion with the air-spark terminal gives another set of effects. Of these the feeblest has the appearance of attraction, while the strongest shows an abrupt termination of the positive column in the neighbourhood of the point, followed by a negative halo, and then by a recommencement of the positive column in the direction of the negative terminal. Each of these sectional discharges is in fact independent and complete in itself, and they are due to impulses of positive electricity thrown into the tube from the air-spark. At the positive terminal these impulses are thrown directly in; at the points of connexion they are due to induction, *ab extra*. The negative part of what was originally neutral meets the positive column, and satisfies it as it arrives, while the positive leaps forward to meet the negative due from the negative terminal.

The effects above described need not be confined to a single patch or ring of conducting material placed upon the tube; but they may be produced many times over in the same tube by a series of rings arranged at suitable distances. By this means the column may be broken into a series of sections, all terminating with well-defined configurations towards the negative end, and having greater or less length, according to the position of the rings. In the paper itself, arguments are there brought forward showing that these sectional discharges represent striæ not merely in their appearance, but also in their function and structure. But the discussion could hardly be produced within the limits of an abstract.

Returning from the digression about striæ, the authors next give evidence, derived mainly from the revolving mirror, and from the discharges of a partially charged Leyden jar, for the following conclusion: That the passage of the discharge occupies a time sufficiently short in comparison with the interval between the discharges to prevent any interference between successive pulses. Certain experiments are then described which indicate that the discharge is effected, under ordinary circumstances, by the passage through the tube from the air-spark terminal of free electricity, of the same name as the electricity at that terminal. In the case of an induction coil, where the air-spark must be considered as existing at both terminals, there is evidence of a *neutral zone*, where the sensitiveness disappears. The position of this zone may be altered by damping the impulses at either terminal; or it may be abolished by connecting one terminal with earth. The impulses may even be so distributed as to divide electrically a single tube into three sections, the two extremes presenting visible discharges, with a dark section between them.

Looking at all these phenomena from an opposite point of view, we may, by means of the relief effects, determine the terminal from which a discharge proceeds, and the distance to which it reaches without provoking a response from the other. And through these considerations, together with others detailed in the paper, the authors are led to the conclusion that the discharges at the two terminals of a tube are in the main independent, and that they are each determined primarily by the conditions at their own terminal, and only in a secondary degree by those at the opposite terminal.

In illustration of this view, an account is then given of the production of unipolar, positive, or negative discharges in a tube. In such cases, the discharge being insufficient of itself to pass through the tube, returns by the way by which it entered.

This closes a series of experiments, the result of which is that the discharges from the two terminals can be made of equal intensity, or of any required degree of inequality; or the discharge can be made to issue from one terminal only, the other acting only receptively; or

it can be made to return into its own terminal, while the other takes no part in the discharge; or, finally, the two terminals can be made to pour out independent discharges of the same name, each of which returns to its own terminal.

Having traced the relation between the two parts of the discharge, and having found means for controlling their range and influence, the authors were led to inquire whether there be any experimental evidence of the state of the tube during the occurrence of the discharge. Some experiments with two pieces of tinfoil of unequal size placed near the ends of the tube and metallically connected; and others with a strip of tinfoil placed along the tube, all gave effects showing that the discharge cannot be simultaneous throughout the tube. The phenomena appear to require for their interpretation that, in front of the pulse coming from the (positive) air-spark terminal, there is, during the interval between the pulses, a rising negative potential. This is entirely swept out by the pulse as it advances along the tube; after which the process is repeated. The condition of things behind the pulse is more difficult to determine; but an experiment with the telephone gives reason to think that parts of the tube nearer to the non-air-spark end are in a condition to demand relief, before those nearer to the air-spark terminal have ceased to require it. And on this account the discharge may, perhaps, be more nearly represented by a lazy tongs than by a bullet.

How far the results obtained from the sensitive state are applicable to ordinary discharges is a question which cannot yet be definitively answered. But the marked similarities in the phenomena, and the predisposing circumstances of striation or non-striation, as well as in the terminal peculiarities of the two kinds of discharge, point strongly to the conclusions that all vacuum discharges are disruptive; and that sensitive differ from non-sensitive discharges mainly in the scale of the discontinuity due to the disruptiveness, causing a difference between the two classes of phenomena analogous to that between impulsive and continuous forces in dynamics.

II. "On the Action of Solid Nuclei." By CHARLES TOMLINSON, F.R.S. Received April 22, 1879.

It is stated in my second paper on supersaturated saline solutions ("Phil. Trans.," 1870, p. 53), that among nuclear bodies "are permanently *porous* substances, such as charcoal, coke, pumice, meerschaum," also that "certain liquids act as nuclei by separating water instead of salt from supersaturated solutions. Absolute alcohol acts in this way."

Some eminent observers have denied the accuracy of these statements. Thus, M. Viollette concludes from his experiments that porous bodies, such as pumice, calcined baryta, lime, and gypsum, and bodies greedy of water, and capable of being hydrated, such as calcined sulphate of iron and of copper, have no influence on supersaturated solutions.

This observer, believing, as he did, that the only nucleus is a salt of the same kind as that in solution, or one isomeric therewith, and that such nuclei are generally floating in the atmosphere, so ingeniously arranged his apparatus as to exclude the action of the air, and hence his results were of a negative character.

Mr. Liversidge ("Proceedings," vol. xx), actuated by the same idea, conducted his experiments on a similar principle, namely, that of excluding the air, and he also arrived at negative results. He employed several solid dehydrating substances, such as freshly ignited quicklime, and placed them in thin glass bulbs, which, being sealed, were heated nearly to redness, and dropped into the supersaturated solutions. The flasks were plugged with cotton wool, through which a glass rod was passed, and were then boiled up and allowed to cool during some hours; when quite cold, the bulb was broken by means of the glass rod, and its contents were set free, but in no case with any result. He also found by his mode of experimenting that porous bodies and absolute alcohol are inactive.

In the "Proceedings" vol. xxvii, p. 122, some account is given by me of a series of observations, extending over several months, on the action of essential oils on a supersaturated solution of sodic sulphate. It was found that when the wind at Highgate was in a southerly or westerly quarter, the oils were inactive; but that they quickly became active when the wind was northerly or easterly.

A similar course of observations, though not extending over so long a time, has led to a similar conclusion respecting solid porous, and dehydrating bodies.

The solutions chiefly employed in the experiments that led me to this conclusion were those of sodic sulphate, sodic acetate, and alum, of various degrees of supersaturation. The solid nuclei employed were chiefly pumice and plaster of Paris; but others were also used, such as meerschaum, marble, lime from marble and chalk, coke, charcoal, and bread.

The flasks containing the solutions were sometimes carried into the open air of my garden before being uncovered. The plaster of Paris was taken out of a bottle by means of a small, clean, platinum spoon, and a fragment of pumice, about the size of a pea, was taken up with forceps, and thus dropped into the solution, the cover being immediately restored to the flask.

The active solid nuclei could be immediately rendered inactive by

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exposing them for a short time to the heat of the kitchen oven or to the flame of a spirit lamp, or even to the temperature of boiling water. For this last purpose a test-tube was thrust through a flat cork, resting on the mouth of a large flask containing water, which was kept boiling. The bottom of the tube containing the plaster, &c., did not pass beyond the centre of the flask. In about ten minutes the nuclei were thus rendered inactive, and they did not recover their activity in atmospheres of carbonic acid, nitrogen, hydrogen, and nitrous oxide, after several days or even weeks. But they frequently became active in oxygen, and generally so in oxygen or air ozonised by the action of phosphorus, and always so by a short exposure to the outer air, when the wind was northerly or easterly. Inactive plaster of Paris has been exposed all night to the air of my garden during a south-west wind, without becoming active. Fragments of pumice exposed to the air of my laboratory for days together, without becoming active, have regained their activity within ten minutes on being exposed to the open air during a northerly wind. Snow was also found to be active.

The details of a few experiments may not here be out of place. The wind at Highgate, from the 28th March to the 8th April, was southerly and westerly. During all this time some plaster of Paris, rendered inactive by heat, was freely exposed on a flat glass plate to the open air of my garden, and remained inactive. On the 9th the wind passed round to the east, and remained so during some hours, gently blowing; the plaster became active on solutions of sodic sulphate and sodic acetate.

Fragments of pumice, meerschaum, and coke, rendered inactive by the heat of the oven, were put into carbonic acid on the 28th March, where they remained until the 9th April, when they were still found to be inactive on solutions of sodic sulphate. They were exposed to the air of my garden, and became active in rather more than two hours.

A similar result was obtained with inactive fragments that had been kept in nitrous oxide.

Mr. Grenfell ("Chemical News," xxxix, 16) is of opinion that the action of nuclei in bringing about the solidification of these solutions is entirely due to absorption. In one of his ingenious experiments, filtering paper, folded a number of times into a long spill, is passed down the neck of the flask so as to touch the solution, the effect being to render it solid either at once or after some time.

I repeated this experiment, with this modification, namely, that of placing the spills in the kitchen oven for about a quarter of an hour; they were then passed down the narrow necks of some flasks which they fitted with friction, and the ends were made to dip just below the surface of a solution of sodic acetate (3 to 1). One of the flasks was

immediately covered with a jar of nitrogen and the other with a jar of air. After three days the solutions were still liquid. The spills were then removed, and fresh ones, not previously heated, were introduced, when one solution immediately became solid, and the other some time after.

If the filtering paper, pumice, plaster, &c., acted by absorption, then the effect of heat would be to increase the absorptive power, and consequently their activity as nuclei; whereas the contrary result is obtained, so that absorption does not seem to be a *vera causa*.

When it is stated that the nuclei became active on being exposed to certain winds in preference to others, the locality in which the experiments were conducted must be taken into account; for, as Professor Stokes suggested at the reading of my former paper, a westerly or southerly wind might be equally active in another place. I found this to be the case at Eastbourne, where a westerly wind produced, in the course of ten minutes, a deep orange-brown stain on ozone test-paper, and made the oils singularly active after a few minutes' exposure. For example, on the 10th of June oil of lemons was inactive in the house, but on being exposed to a strong south-west wind from the sea, about a mile away from the town, it became viscid in the course of a few minutes' exposure, so that it dropped very slowly. The solutions, on contact with this oil, immediately became solid. On the 11th, a limpid paraffin oil, on being redistilled, was inactive within doors. It was taken to the end of the pier, 1,012 feet from the shore, and exposed in drops to a strong westerly wind for about three minutes, when it became powerfully active. On the 13th there was scarcely any wind, but what there was was west south-west. Freshly distilled oil of cajuput was inactive within doors, but, on being exposed to the outer air for about fifteen minutes, it became active.

In July, at Bournemouth, the weather was sultry; ozone test-paper was scarcely acted on, even after an exposure of some hours, and inactive oils did not become active under the influence of the air.

In September, at Denchworth, near Wantage, ozone was abundant, judging from its action on test-paper. Some inactive turpentine oils thickened on being exposed to the air (wind north and south-east), and the effect on the solutions was decisive.

Before concluding I must refer to the nuclear action of alcohol. In order to test this once more, a day was selected when the wind was in the east, when rain had fallen during some hours, and continued to fall all the time the experiments were being conducted in the open air of my garden. About ten flasks of sodic sulphate (3 to 1), and about the same number of potash alum (4 to 3) were prepared. In filling the belly of the flasks about half full, a small funnel with a long stem was used, and was dipped into warm water after each filling, so as not to soil the necks of the flasks; and in reboiling, each flask was

placed at an angle of about 45°, so that if any spitting occurred it would be against the belly of the flask and not into the neck. The necks were covered with small beakers, taken out of water, and the flasks were thus conveyed to the outer air and left to cool, the temperature being 40°. When cold, some absolute alcohol in a small clean beaker was taken up by means of a clean dropping tube, and a single drop or a number of drops was delivered to the surface of each solution without contact with the interior of the neck, the covering beaker, in each case, being lifted a little on one side so as not completely to uncover the mouth of the flask or admit the rain. The sodic sulphate solution in several cases became solid directly the alcohol reached it; in other cases after some minutes; in two flasks the alcohol acted by determining a copious deposit of the modified salt,* but the solution in one of them became solid on gently shaking it, and in the other equally so on adding a few more drops of alcohol. In the case of the alum solution the results were similar. As soon as the alcohol reached the surface there was a sort of trembling motion, as if the surface tension had been disturbed, and also a kind of viscous disturbance in the solution (phenomena equally applicable to the sodic sulphate solutions), when suddenly a single octohedral crystal would start into existence just below the surface; or, if a number of drops of alcohol had been added, a multitude of minute octohedra would all at once appear at the surface plane between the two liquids; or, thirdly, the action would begin from the bottom, and a large crystalline mass be formed, while heat currents rapidly ascended; or, fourthly, on gently shaking the flask, an immense assemblage of chalky-white points filled the solution and made it opaque. In no case did the alcohol fail to exert a nuclear action.

By way of contrast with the above results, the experiments were repeated on another wet day, when the wind was veering between south-east and south-west. The alcohol was now inactive so far as the solidification of the solutions was concerned, but active in throwing down a large quantity of the modified salt. The day after this the wind blew steadily from the north, and alcohol, ether, naphtha, and wood spirit were all active on solutions of sodic sulphate and potash alum, the former becoming solid immediately, and the latter within two or three minutes.

Now, if it be still asserted that whenever these solutions become

* Löwel, in his first memoir, says that if alcohol be introduced into a supersaturated solution of sodic sulphate, it immediately produces the ten-atom or normal salt with solidification of the solution; but if a large quantity of alcohol, at 36° to 40° C., be introduced, crystallization does not take place on cooling. If now the flask be left to repose, the alcohol takes water from the solution, concentrates it, and in two or three days produces the seven-atom or modified salt in fine crystals on the surface of the liquid in contact with the alcohol.

solid, a salt of the same kind is the nucleus, and that such a nucleus is present in the air, I must leave the proof of the assertion to those who make it or defend it. I cannot imagine what can be the source of the alum nucleus, for example, in the above experiments, limited as they were by such well-defined conditions. M. Gernez can only account for the existence of alum in the air on the supposition that as a good deal of that salt is used in dye-works, it must escape into the air from such a source. Until it can be proved that on a wet day and while rain is still falling there is alum in the open air, I must continue to maintain my original statement that alcohol is, under proper conditions, capable of determining the solidification of these solutions.

I have several times noticed the persistence of nuclear action on the part of a dense charcoal, such as that made from box-wood. A small piece of such charcoal was boiled in a solution of potash alum (4 to 3). When cold, a large crystalline mass was found attached to the charcoal. The flask was reboiled five times in the course of a week, and each time the same result appeared when the flask had become cold.

I have referred to bread as a nucleus, and the action is curious. New bread is not nuclear; but if stale bread, such as a second day's loaf, be cut open with a clean knife, and bits of the crumb be dug out, they act powerfully as nuclei on solutions of sodic sulphate and alum. If such bread be slowly toasted before the fire, it becomes inactive; but if it contain alum, it is active on solutions of potash or ammonia alum (about 1 to 1), whether the bread be new or stale, toasted or not. Treated by this test, I have found the bread of Highgate to contain alum; but some bread from Drury Lane gave most decided evidence of its presence.

III. "On the Results of the Magnetical Observations made by the Officers of the Arctic Expedition, 1875–76." By Staff-Commander E. W. Creak, R.N., attached to the Admiralty Compass Department. Communicated by Captain F. J. O. Evans, C.B., F.R.S., Hydrographer of the Admiralty. Received April 25, 1879.

The following narrative and results form the sequel to the "Memorandum on Terrestrial Magnetism," prepared by Professor J. C. Adams, M.A., F.R.S., and Captain F. J. Evans, R.N., F.R.S., published in the "Manual and Instructions for the Arctic Expedition, 1875," suggested by the Arctic Committee of the Royal Society.

The "Alert" and "Discovery" left Portsmouth on the 29th May, 1875, and on arrival at Godhavn, in Disko, the first magnetic observations were made. The values of the declination and inclination

observed at various stations round the harbour of Godhavn, showed considerable differences, evidently caused by local magnetic disturbance.

Proceeding northward to winter quarters a few results in declination were obtained at intermediate stations.

On the 1st September, 1875, the "Alert" reached her winter quarters at Floeberg Beach, latitude 82° 27′ N., longitude 61° 22′ W. The magnetic observatory, constructed entirely of snow, was situated at a distance of 200 yards from the ship. It was divided into three distinct houses 27 feet apart, and in these the horizontal force magnetometer, Barrow's dip-circle, and the differential declination magnetometer were secured to snow pedestals. As no stove was used the range of the thermometer registered was from $-10°$ to $+4°$.

Here the series of observations, the results of which are given in the following pages, were made during the winter. The mean values of the several elements obtained are:—

Absolute declination.....	° 97	′ 57 W.	
„ inclination.....	84	42 N.	
„ horizontal force (unifilar).....	1·137	.... Total force =	12·309
Total force (Lloyd's needles).....			=12·258
Horizontal force (ditto)	1·134		

In his sledging journey to the extreme northerly position attained in the expedition (lat. 83° 20′ 26″ N.), Commander Markham made observations of the declination, and with the Fox circle.

The "Discovery" took up winter quarters at Discovery Bay, 53 miles S.W. of the "Alert," in latitude 81° 44′ N., longitude 65° 3′ W., on the 26th August, 1875.

The magnetic observatory house was constructed of wood, and stood on the shore 197 yards from the ship, and the pedestals formed of the ship's binnacles sunk into the ground were found to be very firm. For the differential declination magnetometer an ice-house was built on the ice-floe 90 yards from the ship, the magnetometer being frozen on to a pedestal of ice. The mean value of the several elements observed at this station are:—

Absolute declination.....	° 101	′ 44 W.	
„ inclination.....	84	50 N.	
„ horizontal force (unifilar).....	1·131	.... Total force =	12·559
Total force (Lloyd's needles).....			=12·225
Horizontal force (ditto)	1·107		

One of the chief subjects of interest in the magnetical results of the observations at the winter quarters observatories is that of the diurnal variation or inequality of the declination, and frequent magnetic disturbances, the latter especially, as the ships wintered in a region

remarkable, as it proved, for an absence of brilliant auroras, and in which no connexion was observed between the appearances of that phenomenon and movements of the declinometer magnet.

Hourly observations were made with the differential declination magnetometers, and on all occasions of great disturbance special observations at frequent intervals. At Floeberg Beach continuous hourly scale readings were taken during portions of December, 1875, and January and February, 1876—in all 37 days; and at Discovery Bay from the 23rd October, 1875, to the 28th March, 1876, with only a break of two days in November, and a few hours in February.

The greatest range of the declination was observed on the 19th February, 1876, about the same hours, at Floeberg Beach and Discovery Bay, reaching 5° 9′·4 and 5° 47′·9 respectively. The smallest range was observed on the 12th January, when only 0° 4′ was recorded at the first-named station, and 0° 6′·9 at the last. On an average, about every eighth day the higher values of the daily range were attained, and comparing the highest with the lowest scale reading during the whole period, it shows that the magnet moved over 8° of arc.

The mean daily ranges for the several months or the square root of the mean of the squares of the daily ranges for each month expressed in arc, are :—

Floeberg Beach.		*Discovery Bay.*	
1875. December	= 71′·4	1875. October	= 93′·3
1876. January	= 68·5	„ November	= 90·2
„ February	=110·9	„ December	= 69·6
		1876. January	= 72·3
		„ February	= 93·7
		„ March	=108·2

At Discovery Bay, therefore, the mean daily range gradually declined in value from October to December, when the minimum range was reached, but again increased during the subsequent months of observation.

Comparing the results of the mean daily range during the whole period with those obtained at Floeberg Beach and Van Rensselaer Harbour *—stations where the inclination and horizontal force differ so little from the same elements at Discovery Bay—it is found that the mean daily range at Van Rensselaer Harbour was 2° 28′·6; Floeberg Beach, 1° 31′·5; Discovery Bay, 1° 28′·0.

The hours of the greatest easterly and westerly deflection of the declinometer magnet in its mean daily departure from the normal, differed considerably from those of the middle northern latitudes, where 8 A.M. and 1 to 2 P.M. are the well-established hours.

* Van Rensselaer Harbour lies S. 17° W. 196 miles from Discovery Bay. See "Smithsonian Contributions to Knowledge," vol. x, 1858.

The extreme deflections of the magnet (always regarding the mean position of the north end as being the magnetic north) took place, at

Floeberg Beach.

Westerly extreme.	December, 10 A.M., January, noon, February, 1 P.M.	Easterly extreme.	December, 1 A.M., January, 9 P.M., February, midnight.

Discovery Bay.

Westerly extreme.	October, 11 A.M., November, 10 A.M., December, 10 A.M., January, 1 P.M., February, noon, March, noon.	Easterly extreme.	October, 11 P.M., November, 11 P.M. December, midnight, January, 10 P.M., February, 11 P.M., March, midnight.

As Van Rensselaer Harbour and Port Bowen, as well as Floeberg Beach and Discovery Bay, happen to lie in a region within which observers hitherto report their inability to discover any connexion between appearances of the aurora and magnetic disturbances, the mean hours of extreme deflection of the magnet for the whole period are here given for comparison:—

Westerly extreme.	Noon.....	Van Rensselaer Harbour..	2 A.M.	Easterly extreme.
	Noon.....	Floeberg Beach	Midnight	
	10 A.M.....	Discovery Bay	11 P.M.	
	11·49 A.M...	Port Bowen	10 P.M.	

As it is probable that magnetic disturbances have had a certain degree of control ever the determination of these hours of extreme deflection, it now becomes necessary to give results freed from such disturbance.

The method of eliminating disturbances adopted in the discussion of these observations, has been to select certain days of principal disturbance, and to reject the observations of the whole of such days from the mean.

The observing officers recorded 32 days of undoubted disturbance.

An examination of the hourly scale readings on these days shows that the motions of the magnet were very irregular from hour to hour, and that there were other occasions when similar movements took place and might likewise be considered days of disturbance. Thus from the 37 days of observation at Floeberg Beach, 18 days were rejected as disturbed, and at Discovery Bay 69 days out of 156.

But another method, which consists in deciding upon a separating value and rejecting all hourly observations exceeding that value, may also be said to have been satisfied, for out of 2,088 hourly scale readings at Discovery Bay, all those that exceeded the mean hourly value for the whole month by 30′ of arc have been rejected, with the exception of 15 which differed 40′ of arc.

A separating value of 30′ at a station where the absolute horizontal force is 1·13, and the inclination 84° 50′, may be considered sufficient, when compared with those adopted by Sir Edward Sabine, viz., at Point Barrow, 22′·87, with a horizontal force equal 1·88, and dip = 81° 36′, at Port Kennedy, 70′ with horizontal force equal 0·35, and dip = 88° 27′.*

After rejecting days of assumed disturbance the hours of extreme deflection are:—

Floeberg Beach.

Westerly extreme.	December, 1 P.M., January, 2 P.M., February, 3 P.M.	Easterly extreme.	December, 1 A.M., January, 10 P.M., February, 2 A.M.

Discovery Bay.

Westerly extreme.	October, 11 A.M., November, noon, December, 11 A.M., January, noon, February, 11 A.M., March, 2 P.M.	Easterly extreme.	October, 9 P.M., November, 3 A.M., December, midnight, January, 3·30 A.M., February, 4 A.M., March, 9 P.M.

The mean values for the whole period are.—

Westerly extreme.	1 P.M.	Van Rensselaer Harbour	2 A.M.	Easterly extreme.
	10 A.M. and 1 P.M.	Floeberg Beach	1 A.M.	
	11 A.M.	Discovery Bay	Midnight	

The general effect, therefore, of disturbance is to accelerate the arrival of the magnet at its points of extreme deflection by one hour, in addition to producing a much larger range in the diurnal variation. Thus, with disturbances included, the range is 35′·6; with their rejection it is only 16′·3 at Discovery Bay.

The erection of the declinometer houses of both ships on the ice-floe (which rose and fell with the tide), renders the observed motions of the magnet open to the suspicion of disturbances other than those produced by magnetical causes.

Chiefly for this reason no further analysis has been made of the disturbances at Floeberg Beach.

In the case of the "Discovery," comparisons were made between the declinometer on the floe and the unifilar magnetometer on shore during times of disturbances, and the magnets moved alike, with two exceptions. On February 2nd, the magnet on shore was much disturbed, that on the ice very steady; at midnight of February 28th, the reverse took place, the declinometer being disturbed in a peculiar manner, whilst the unifilar magnetometer remained nearly steady.

Analysis of Disturbances.

For computing the mean hourly values of the disturbances of the

* See "Phil. Trans.," 1863, p. 656.

declination, the formula used by Captain (now Sir J. H.) Lefroy, in the published volume of his "Magnetical and Meteorological Observations at Lake Athabasca,"* &c., was adopted. This renders the several results directly comparable with other series obtained at numerous stations on the North American continent, and Van Rensselaer Harbour, Greenland.

The formula for a whole month or longer period is,

$$\sqrt{\frac{\Sigma^1}{N}(\psi h - \overline{\psi h})^2}.$$

In this ψh represents the hourly scale readings, $\overline{\psi h}$ the monthly mean at the same hour, N the total number of the observations, Σ^1 the sum of all the squares.

From the values of the mean hourly disturbance without regard to sign, it is found that the disturbing force never ceases, and that in the mean monthly values it *decreases* as the winter solstice is approached, and *increases* rapidly towards the equinox. This points to an accordance with the investigations by Mr. Broun, F.R.S., of the disturbances of declination in high northern latitudes, "that there are maxima at the equinoxes and minima at the solstices."†

In the hourly values the maximum mean disturbance occurs between the hours of 9 A.M. and 3 P.M., and again from 9 P.M. to 2 A.M.; the minimum from 6 to 9 P.M. and 3 to 8 A.M. Comparing the results for the whole period at Van Rensselaer Harbour and Discovery Bay, a marked difference is apparent.

At the first-named place the disturbing force was greatest during the night and least during the day: at Discovery Bay the disturbing force was considerably greater during the day than the night.

Another result which also deserves attention is that of the mean maximum disturbance about noon, being preceded and followed by smaller values. This is in accordance with the results obtained at Van Rensselaer Harbour, Lake Athabasca, Toronto, and Sitka. At Discovery Bay there is also a very marked disturbance at 11 P.M.

Having considered the mean disturbance without regard to sign, it yet remains to be seen what was the *direction* of the disturbances during the several hours of the twenty-four. The computed values show that the easterly disturbances prevail over the westerly during the night from about 7 P.M. to 7 A.M., and during the remaining hours of the day the westerly disturbances predominate.

Taking the difference between the mean easterly and mean westerly disturbances, it is found that the easterly disturbance is both moderate in amount and monthly change when compared with the westerly.

Thus the ratio of the excess of westerly disturbance in December

* See "Irregular Fluctuations," &c., pages 74–75 of that work.

† See "Trans. Roy. Soc. Edinburgh," vol. xxvii, Part IV, page 586.

to that in March is 3·25, but the ratio of the easterly excess in the same months is 1·32; the mean actual values being −11·7 and −38·1; +8·5 and +11·2.

Among the peculiarities of the disturbances at Discovery Bay, are two which exhibit their semi-American, semi-Asiatic character.

At Pekin, Nertschinsk, and Discovery Bay the aggregate values of the westerly deflections decidedly predominate, the contrary obtains in America. In North America "the conical form (of the curves) characterizes the easterly deflections."* The same occurs at Discovery Bay, but the double maximum—which is observed in the easterly deflections at Pekin and Nertschinsk, and the westerly in North America—is common to both easterly and westerly deflections.

Objections may possibly be raised to a comparison being made between the observations of a few months with those of as many years. The period of observation, however, includes one solstice and the subsequent equinox; it is, therefore, considered that according to Mr. Broun's investigations in high latitudes, all the broad features of the disturbances have been rendered prominent.

Before concluding this analysis, the question of how far the observed and assumed days of disturbance at Discovery Bay coincided with those at Kew Observatory seems worthy of examination.

The period embraced by these observations was one of great disturbance, especially towards the end of February and the greater part of March, the declinometers at both stations being for the most part affected on the same days. At Kew Observatory, Mr. Whipple noted 57 days of principal disturbance, while at Discovery Bay there were 72.

The two greatest disturbances or "magnetic storms," occurred on the 19th February and the 25th March, and the time limits at the two stations agree fairly in "absolute time,"† thus: the principal disturbances—

	Kew.	*Discovery Bay.*
19th February, 1875.	Commenced at 4 to 5 P.M.	Commenced at 4 P.M.
	Ended at 2 P.M. of 20th	Ended between 1 and 2 P.M. of 20th.
25th to 26th March, 1875.	Commenced at 11·45 A.M.	About noon.
	Ended at 3·30 A.M., 26th	Indeterminate.

The hours of disturbance at Floeberg Beach were about the same (in February) as at Discovery Bay.

During the second storm of the 25–26th March at Discovery Bay, the magnet kept continuously to the westward of the normal from the commencement to 10.20 p.m. (absolute), and at 6.50 p.m. (absolute) it had deviated 4° 15′ from the mean hourly position, whilst at Kew it was often to the eastward as well as to the westward, 18′ being the

* See "Phil. Trans. Roy. Soc.," 1863, page 282.

† Or mean time at Greenwich.

greatest decrease in the declination; and at 6.50 p.m. it was 12′ to the eastward of the normal.

Finally, it may be stated that the comparison of the disturbances at Kew and the two winter quarters observatories of the Arctic Expedition, 1875–76, appear to confirm Gauss's conclusion, that "the synchronous disturbances of the same element not only differ widely in amount, but occasionally appear to be even *reversed* in direction."

Auroras.

The appearances of auroras and the synchronous movements of the declinometer magnet, were subjects of special observation during the stay of the "Alert" and "Discovery" at their winter quarters. On all occasions they were observed to be faint, with none of those brilliant manifestations which are described by our own officers as seen at Point Barrow,* and by the Austro-Hungarian Expedition in Franz Josef Land,† where the magnetical instruments were so sensibly disturbed.

These phenomena were not observed either in the "Alert" or "Discovery," especially no connexion between magnetical disturbances and the appearances of auroras could be traced.

This is quite in accordance with the remarks of previous observers within the region comprehended between the meridians of 60° and 90° W., and north of the parallel of 73° N. For examples, see the remarks of Captain Parry and Lieutenant Foster in the discussion of their magnetical observations at Port Bowen, in the "Phil. Trans.," 1826, Part IV, page 76. Also Mr. A. Schott's discussion of Dr. Kane's observations at Van Rensselaer Harbour, "Smithsonian Contributions," vol. x, 1858.

The following description of the aurora observed on 21st November, 1875, is given by Commander Markham and Lieutenant Giffard in their abstract of observations at Floeberg Beach:—

"Between 10 and 11 p.m. bright broad streamers of the aurora appeared 10° or 15° above the north horizon, stretching through the zenith, and terminating in an irregular curve about 25° above the south horizon, bearing S.S.W. During the aurora's greatest brilliancy the magnet was observed during five minutes to be undisturbed."

The aurora was visible on 49 days between the 22nd October, 1875, and the 27th February, 1876.

The observations from which the foregoing magnetical results have been obtained were made by Commander (now Captain) A. H. Markham and Lieutenant G. A. Giffard, of the "Alert," Lieutenants R. H. Archer and R. B. Fulford, of the "Discovery."

The results in greater detail will be found in a Parliamentary paper

* See "Phil. Trans. Roy. Soc.," 1857, page 498.

† See "Austrian Arctic Voyage," 1872–74, pages 327–328.

entitled "Arctic Expedition," "Results derived from the Arctic Expedition, 1875–76," presented to the Houses of Parliament in the session of 1879.

H.M.S. "Alert."

Declination.

Godhavn, Disko.

1875.			°	′	
9 July..	7.0 P.M...	(Flagstaff) Lat. 69° 13′ 56″ N. Long. 53° 42′ W.	67	12·8	W.
13 „	.....	(Macdougall Island)	68	7·9	„

Ritenbenk.

16 „ ..	6.40 P.M.	Lat. 69° 45′ 23″ N. Long. 51° 7′ 45″ W.....	69	8·5	„

*Proven.**

21 „ ..	4.20 P.M.	Lat. 72° 22′ N. Long. 55° 45′ W.	75	32·0	„

East Carey Islands.

27 „ ..	8.40 A.M.	Lat. 76° 42′ N. Long. 72° 25′ W.	105	20·7	„

Port Foulke.

28 „ ..	5.40 P.M.	Reindeer Point.....	110	4·0	„
		Lat. 78° 19′ N. Long. 72° 55½′ W.			
9 Aug...	5.40 P.M.	On ice	110	49·2	„
			109	14·5	„
11 „ ..	6.15 P.M.	Off East Cape of Pierce Franklin Bay.....	111	13·2	„
		Lat. 79° 25′ N. Long. 75° 3′ 15″ W.			

Dobbin Bay.

14 „ ..	6.30 P.M.	Lat. 79° 40′ N. Long. 73° 6′ 15″ W.....	107	25·2	„

On Ice off Hayes Point.

16 „ ..	10.40 A.M.	Lat. 79° 36′ N. Long. 72° 24′ W.	111	23·0	„

Winter Quarters Observatory.

10 Sep...	10·5 A.M...	Lat. 82° 27′ 2″ N. Long. 61° 22′ W.....	98	16·9	„
„ „ ..	4.15 P.M.		97	57·5	„
11 „ ..	5.30 P.M.		97	48·0	„
	December mean from Declinometer (for differential observations)		97	46·6	„
1876. January	„	„ „	98	01·8	„
February	„	„ „	97	51·7	„

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* On a small hill just left of Flagstaff. A rock at the place where the variation was observed had the marks cut into it--

xxi
July
1875
↑
H.M.S.
Alert.

Inclination.

1875.			
May.....	Kew Observatory (departure)	67° 47′·54 N.	

Godhavn, Disko.

July.....	Lat. 69° 13′ 56″ N. Long. 53° 42′ W.....	81 43·7	„

Winter Quarters Observatory.

November	Lat. 82° 27′ 02″ N. Long. 61° 22′ W.....	84 43·4	„
December	„ „	84 44·4	„
1876.			
January.....	„ „	84 40·5	„
February	„ „	84 39·2	„
March.....	„ „	84 39·9	„
1877.			
2 Feb. .. 2.30 P.M.	Kew Observatory (return)	67 43·9	„
3 „ .. 11.30 A.M.	Kew Observatory	67 44·2	„

Horizontal Force.

1875.			
May.....	Kew Observatory (departure)..	X=3·8762	Values of *m*.

Winter Quarters Observatory.

November ..	Lat. 82° 27′ 02″ N. Long. 61° 22′ W. ..	„ =1·133	·6396
December ...	„ „	„ =1·1375..... ..	·6374
1876.			
January	„ „	„ =1·140	·6370
February....	„ „	„ =1·140	·6371
March	„ „	„ =1·140	·6376
1877.			
January	Kew Observatory (return)	„ =3·888	·6264

At the Winter Quarters Observatory the mean values of P or coefficient of magnetic distribution for the unifilar magnetometer were at distances 1·1 to 1·3 feet—·0212; at distances 1·2 to 1·4 feet—·0161. With the temperature at + 4° F., $\log \pi^2 k = 1{\cdot}66177$.

Assuming the mean inclination = 84° 42′ N. during the period when the horizontal force was observed as above, the total force = 12·309 at Winter Quarters Observatory.

Total Force.

(Observed with Lloyd's needles on Barrow's Circle.)

1875.			
July.....	Godhavn, Disko (Flagstaff Station)..... Lat. 69° 13′ 56″ N. Long. 53° 42′ W.	12·530.....	X=1·805

Winter Quarters Observatory.

November ..	Lat. 82° 27′ 02″ N. Long. 61° 22′ W.....	12·215.....	„ =1·122
December ..	„ „	12·198.....	„ =1·119

1876.			
January	Lat. 82° 27′ 02″ N. Long. 61° 22′ W.....	12·303.....	X =1·143
February....	„ „	12·260.....	„ =1·142
March	„ „	12·316.....	„ =1·145
1877.			
February....	Kew Observatory (return)	10·273.....	„ =3·891

In computing the horizontal from the total force the values of the inclination have been taken from those observed at the time of the total force observations.

Observations of Declination made with Sledge Compass near the Extreme Northerly Position attained in the Expedition.

1876. 6 May	Lat. 83° 16½′ N.	Long. 62° 40½′ W.	102° W.
„ 8 „	Lat. 83° 17½′ N.	Long. 62° 40½′ W.	98° W.

Results of Observations made with Fox's Circles.

Inclination.

1875.				°	′
9 to 13 July..	Godhavn, Disko (Flagstaff Station)	Circle,	No. 28..	81	55 N.
	Lat. 69° 13′ 56″ N. Long. 53° 42′ W. ..	„	No. 2 ..	81	44 „
16 July	Ritenbenk:				
	Lat. 69° 45′ N. Long. 51° 08′ W.	„	No. 28..	82	00 „
21 „	Proven:				
	Lat. 72° 22′ N. Long. 55° 45′ W.	„	„	82	48 „
1876.					
8 May	Northern Journey.....	„	No. 2.		
	Lat. 83° 17′ N. Long. 62° 40′ W.	„	„	84	39 „ (Approx.
11 May.....	Lat. 83° 19′ N. Long. 62° 39′ W.	„	„	85	20 „ (Approx.)

Intensity.

1875.				
9 to 13 July..	Godhavn, Disko (Flagstaff Station)	Circle,	No. 28..	12·561
	Lat. 69° 13′ 56″ N. Long. 53° 42′ W. ..	„	„ ..	12·578

H.M.S. "Discovery."

Declination.

				°	′
1875. 8 July....	5 A.M...	Godhavn, Disko		68	7 W.
„ 9 „	10 A.M...	Opposite side of harbour to Flagstaff ..		68	8 „
„ 12 „	3.30 P.M.	Lat. 69° 14′ N. Long. 53° 42′ W.		68	45 „
		Carey Islands.			
„ 27 „	5.10 A.M.	Lat. 76° 43′ N. Long. 72° 25′ W.		105	7 „
		Cape Sabine.			
„ 31 „	4.0 P.M.	Lat. 78° 42′ N. Long. 74° 20′ W.		107	59 „

			On Ice.			
1875.	7 Aug.....	2.45 P.M.	Lat. 79° 16′ N. Long. 74° 10′ W.	110	18	W.
			Cape Prescott.			
,,	9 ,, ...	8.0 P.M.	Lat. 79° 26′ N. Long. 74° 45′ W.	110	59	,,
,,	11 ,, ...	5.40 P.M.	,, ,, ...	109	33	,,
			Dobbin Bay.			
,,	14 ,, ...	4.0 P.M.	Lat. 79° 41′ N. Long. 72° 53′ W.	108	14	,,
			On Ice (near C. Collinson).			
,,	21 ,, ...	4.0 P.M.	Lat. 80° 3′ N. Long. 70° 22′ W.....	106	1	,,
			Hannah Island.			
,,	24 ,, ...	0.45 P.M.	Lat. 81° 7′ N. Long. 63° 53′ W.....	97	46*	,,
			Winter Quarters Observatory.			
,,	9 Nov.....	10.0 A.M.	Lat. 81° 44′ N. Long. 65° 3′ 14″ W.....	101	43	,,
		5.45 P.M.	,, ,, ...	101	13	,,
,,	23 Dec.....	5.30 P.M.	,, ,, ...	101	49	,,
		6.30 P.M.	,, ,, ...	101	44	,,
1876.	21 Jan. ..	5.30 P.M.	,, ,, ...	101	26	,,
,,	17 Feb. ..	5.30 P.M.	,, ,, ...	101	14	,,
,,	17 ,, ..	7.0 P.M.	,, ,, ...	101	50	,,
,,	22 ,, ..	Noon ..	,, ,, ...	101	53	,,
,,	1 March*	4.30 P.M.	,, ,, ...	102	7	,,
,,	22 ,, ..	4.30 P.M.	,, ,, ...	101	33	,,
,,	3 July		,, ,, ...	101	47	,,
1875.	October	Mean from Declinometer (for differential observations)		102	02·5	,,
,,	November	,, ,, ,,		101	49·5	,,
,,	December	,, ,, ,,		101	47·5	,,
1876.	January	,, ,, ,,		101	44·4	,,
,,	February	,, ,, ,,		101	40·2	,,
,,	March ..	,, ,, ,,		101	40·7	,,
			Cape Frazer.			
,,	24 Aug. ..	10.40 A.M.	Lat. 79° 47½′ N. Long. 71° 19′ W.....	108	59	,,
			(Near) C. Louis Napoleon.			
,,	25 ,, ..	10.30 A.M.	Lat. 79° 40′ N. Long. 72° 5′ W.....	109	46	,,
			Dobbin Bay.			
,,	29 ,, ..	10.4 A.M.	Lat. 79° 41′ N. Long. 72° 53′ W.	109	1	,,
			Walrus Island (on Ice).			
,,	9 Sept. ..	0.50 P.M.	Lat. 79° 24′ N. Long. 74° 45′ W.	111	12	,,
			Godhavn, Disko.			
,,	27 ,, ..	11.45 A.M.	Lat. 69° 14′ Long. 53° 42′ W.....	68	20	,,

* Doubtful result.

Egedesmunde.

				°	′	
1876. 29 Sept. ..	0.15 P.M.	Lat. 68° 43′ N. Long. 52° 54′ W.		67	2	W.

Inclination.

1875.					
May.....	Kew Observatory (departure)		67	47·5	N.
	Godhavn, Disko.				
July.....	Lat. 69° 14′ N. Long. 53° 42′ W.		81	48·2	„
	Winter Quarters Observatory.				
September	Lat. 81° 44′ N. Long. 65° 3′ 14″ W.		84	49·9	„
October	„ „		84	45·5	„
November	„ „		84	47·9	„
December	„ „		84	49·9	„
1876.					
January.....	„ „		84	50·2	„
February	„ „		84	50·8	„
March	„ „		84	53·7	„
April	„ „		84	48·2	„
June	„ „		84	55·9	„
July.....	„ „		84	53·6	„
	Rawlins Bay.				
21 Aug... 9.0 P.M. ..	Lat. 80° 21′ 5″ N. Long. 70° 0′ W.....		85	6·5	„
	Dobbin Bay.				
29 „ .. 5.0 P.M. ..	Lat. 79° 41′ N. Long. 72° 53′ W.		85	14·9	„
	Egedesmunde.				
29 Sept. 1.0 P.M. ..	Lat. 68° 43′ N. Long. 52° 54′ W.		82	0·2	„
1877.					
14 Feb... 11.0 A.M. ..	Kew Observatory (return)		67	44·7	„

Horizontal Force.

1875.			
May.....	Kew Observatory (departure).....	X=3·878 ..	Value of *m*.
	Winter Quarters Observatory.		
October	Lat. 81° 44′ N. Long. 65° 3′ 14″ W.	„ =1·133	·6539
November ..	„ „	„ =1·132	·6686
December ..	„ „	„ =1·121	·6630
1876.			
January....	„ „	„ =1·117	·6581
February ..	„ „	„ =1·146	·6686
July	„ „	„ =1·128	·6546
1877.			
14 Feb. .... 3.5 P.M. to 4.24 P.M. ..	Kew Observatory (return)	„ =3.917	·6411

Assuming the mean inclination = 84° 50′ N. during the period when the horizontal force was observed as above at Winter Quarters Observatory, the total force = 12·559.

At Winter Quarters Observatory the mean values of P or coefficient of magnetic distribution for the unifilar magnetometer were at distances 1·3 to 1·6 feet + ·0515 from the observations in October, November, December, and + ·0270 during the last three months. With a mean temperature of + 25°, log $\pi^2 k$ = 1·656981.

Total Force.

(Observed with Lloyd's needles on Barrow's Dip Circle.)

1875.				
12 July....	12 to 1 P.M...	Godhavn, Disko (on opposite side of harbour to flagstaff.) Lat. 69° 14′ N. Long. 53° 42′ W.	12·387	X = 1·770

Winter Quarters Observatory.

October	Lat. 81° 44′ N.	Long. 65° 3′ 14″ W.	12·180	„ = 1·108
November	„	„	12·269	„ = 1·113
December	„	„	12·227	„ = 1·101

In computing the horizontal from the total force the values of the inclination have been taken from those observed about the time of the force observations.

Results of Observations made with Fox's Circle.

Inclination.

1875.					
8 to 12 July	Godhavn, Disko ..... (Station on opposite side of harbour to Flagstaff Station of "Alert.") Lat. 69° 14′ N. Long. 53° 42′ W.	Circle No. 29..	° 81	′ 56	N.
21 „	Proven..... Lat. 72° 22′ N. Long. 55° 45′ W. } Approximate.	„ ..	83	4	„
22 „	Upernivick..... Lat. 72° 47′ N. Long. 56° 3′ N. } Approximate.	„ ..	83	12	„

Intensity.

1 to 12 „	Godhavn, Disko..... (Station on opposite side of harbour to Flagstaff Station of "Alert.") Lat. 69° 14′ N. Long. 53° 42′ W.	„ ..	12·514	

IV. "On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force, as observed at the Royal Observatory, Greenwich, during the years 1841 to 1877, and the Period of Solar Spot Frequency." By WILLIAM ELLIS, F.R.A.S., Superintendent of the Magnetical and Meteorological Department, Royal Observatory, Greenwich. Communicated by Sir GEORGE AIRY, K.C.B., F.R.S., Astronomer Royal. Received April 23, 1879.

(Abstract.)

In this paper the author draws attention to the long series of magnetical observations which have been made at the Royal Observatory, under the direction of Sir George B. Airy, K.C.B., Astronomer Royal. Commencing in the year 1841, the observations for a few years consisted of eye readings of the various instruments, made every two hours; since the year 1848 the motions of the magnets have been registered by photography, according to a plan arranged by Mr. Charles Brooke.

Attention is briefly drawn to the question of magnetic variations, and to the circumstance that examination of the Greenwich records shows that, in addition to the ordinary diurnal and annual changes, there appears to exist, in the magnetic diurnal ranges, an inequality of marked character, and of longer period, resembling in its features the well-established eleven-year sun-spot period.

It is remarked that this is not, by any means, the first time that such relation has been discussed, it being, by some investigators, considered to be already sufficiently well proved. But it appeared to the author that the long series of Greenwich observations might be well applied as an independent test of the accuracy of the supposed relation. For (as regards the results actually employed) the observations have been throughout made on the same general plan, and with the same instruments. The conclusion arrived at should therefore be one worthy of some confidence.

The results for declination and horizontal force only are used, because, the vertical force instrument having been more than once changed, the strict continuity of the record by the latter instrument is somewhat broken.

The monthly mean diurnal range of declination, or of horizontal force, is taken to represent the magnetic energy of the month relatively to other months. Two series of numbers are thus formed, each series being treated independently. Days of unusual magnetic disturbance are omitted.

Before the numbers can be compared with sun-spot numbers, it is

necessary, in order to estimate progressive change, to treat them in some way that will eliminate the actual annual inequality. This is done, both for declination and horizontal force, by taking the mean of each twelve consecutive monthly values, and again the mean of each two consecutive values, giving a series of annual or smoothed means, the mean epochs of which differ by one month.

In the forty-third volume of the "Memoirs of the Royal Astronomical Society," Dr. Rudolf Wolf, of Zürich, gives a table of relative solar spot values, found by treating actual monthly numbers (for the purpose of removing accidental irregularities), exactly in the same way in which the monthly magnetic numbers were treated.

The magnetic numbers and Dr. Wolf's sun-spot numbers are thus strictly comparative, and from these numbers curves are drawn which show a remarkable similarity. Not only do the epochs of minimum and maximum accord, but smaller changes are also seen to appear in all three curves.

The epochs of minimum and maximum being tabulated, it is found that, on the average, the mean magnetic epoch follows the sun-spot epoch by 0·27 of a year. By another method of tabulation the difference becomes reduced to 0·10 of a year. Some other interesting details are here added.

The general circumstance that the diurnal ranges of magnetic elements are subject to an eleven-year period, similar to that of sun-spots, being considered to be satisfactorily established, it seemed desirable to ascertain whether the more fitful changes of the phenomena in any way also correspond. In order to make this comparison, the magnetic numbers, instead of being smoothed, as before described, were now cleared only of the *average* annual inequality, and compared with the *actual* monthly sun-spot numbers (not the smoothed numbers as before). Curves are given, founded on these numbers, and they show some very remarkable correspondences between the more rapid sun-spot and magnetic variations, especially between the years 1869 and 1873. It is remarked that the magnetic results appear to be sensibly improved since the instruments were placed (in the year 1864) in an underground apartment in which the diurnal range of temperature is less than 1°.

Further inquiry seems to point to a variation in the annual inequalities of magnetic diurnal range. This question is discussed in some detail, and the numbers and diagrams given appear to show that such variation really exists, the inequalities being increased at the time of a sun-spot maximum, and diminished at the time of a sun-spot minimum.

The following are the general conclusions supposed to be derived from the whole inquiry:

1. That the diurnal ranges of the magnetic elements of declination

and horizontal force are subject to a periodical variation, the duration of which is equal to that of the known eleven-year sun-spot period.

2. That the epochs of minimum and maximum of magnetic and sun-spot effect are nearly coincident; the magnetic epochs, on the whole, occurring somewhat later than the corresponding sun-spot epochs. The variations of duration in different periods appear to be similar for both phenomena.

3. That the occasional more sudden outbursts of magnetic and sun-spot energy, extending sometimes over periods of several months, appear to occur nearly simultaneously, and progress collaterally.

4. That it seems probable that the annual inequalities of magnetic diurnal range are subject also to periodical variation, being increased at the time of a sun-spot maximum, when the mean diurnal range is increased, and diminished at the time of a sun-spot minimum, when the mean diurnal range is diminished.

Conclusions Nos. 1, 2, and 3 appear to be sufficiently certain, but the evidence in favour of No. 4 is not so decisive.

May 15, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "Note on a recent Communication by Messrs. Liveing and Dewar." By J. NORMAN LOCKYER, F.R.S. Received April 30, 1879.

In my paper of last December* I called attention to the importance of discussing Young's observations of the chromospheric lines in connexion with the spectra of the metallic elements. In subsequent communications I have given preliminary results of this discussion so far as it has already proceeded.

Since my paper was read Messrs. Liveing and Dewar have, in a paper printed in the last number of the "Proceedings," given a table which professes to state the number of times various lines in certain metals were seen by Young in connexion with certain reversal phenomena observed by themselves.

The statements, however, made in this table with regard to the

* "Proceedings," vol. xxviii, p. 172.

visibility of certain lines in the chromosphere do not appear to be in accordance with Young's published tables, and as Messrs. Liveing and Dewar have in a still later paper drawn theoretical conclusions from these statements, I think it desirable to call attention to the fact, in order to prevent any confusion which might otherwise arise.

It will be sufficient to refer to two cases:—

I. Messrs. Liveing and Dewar state that two lines of aluminium, the wave-lengths of which they give as 6245 and 6237, have each been seen by Young eight times.

According to Thalén's measurements, which are the best that we possess, there are no lines of aluminium in these positions. He gives, however, lines at 6244·0 and 6234·0.

Young, moreover, states that he saw reversed a strong line (clearly shown in Ångström's map to be an iron line) at 6245·4, and a line which he does not ascribe to any element at 6237·3, which is more than three divisions of the scale from the position of the aluminium line.

II. In the case of potassium, Messrs. Liveing and Dewar give two lines at wave-lengths 4044 and 4042 as having been seen by Young three times. I know of no potassium lines at the places given; Young moreover has recorded the reversal of no potassium line in this region. What Young distinctly states he saw, was the reversal of the iron line at 4045·0, which is one of the most marked iron lines in the spectrum of the sun. To this reversal I referred in my paper of December 12th.*

It is perfectly true that there are two potassium lines in this region; they were not mapped by Thalén, and they were only seen as a single line by Lecoq de Boisbaudran† and the wave-length, given as 4045, as his dispersion was limited, did not enable us to determine its true position with reference to the Fraunhofer lines. Last year, however, I not only stated the double nature of this line on photographic evidence,‡ and pointed out that both components were absent from the spectre normal, but I gave their wave-lengths as 4042·75 and 4046·28 (positions which will only find the last place of decimals altered, even if it be altered, in the revision of the map now being proceeded with), and on the strength of them announced the existence of potassium in the sun. Messrs. Liveing and Dewar do not state whence their wave-lengths were derived, neither do they refer to my communication. It would appear therefore not only that the reference to Young's work in many cases is founded upon some misunderstanding, but that a higher degree of accuracy than that employed by Messrs. Liveing and Dewar is necessary to determine such coincidences. I may state generally that my eleven years' work on this special branch has led

* "Proceedings," vol. xxviii, p. 172. † "Spectres Lumineux," texte, p. 48.

‡ "Proceedings," vol. xxvii, p. 280.

me to the conclusion that all statements of coincidences between metallic and solar lines with a lower degree of accuracy than that employed by Thalén and Young are to be avoided when possible as they may be worse than useless, they may mislead. Indeed, though the map on which I am working is on twelve times the scale of Ångström's, it would be better if it were larger; and when I say this I must add my tribute of admiration of the accuracy of the work of those who have preceded me, notably Ångström, Thalén, Cornu, and Young, with whose work I am more familiar, as it is expressed in wave-lengths.

II. "Sur la Limite Ultraviolette du Spectre Solaire." Par Mons. A. CORNU. Communicated by the PRESIDENT. Received May 15, 1879.

Je me suis proposé depuis plusieurs années d'obtenir la limite extrême du spectre solaire du côté ultraviolet, c'est-à-dire, du côté des courtes longueurs d'onde. La connaissance exacte de la limite de ce spectre ou plutôt de la loi suivant laquelle décroit l'intensité du spectre continu idéal de la photosphère, apporterait sur la difficile question de la température du soleil des données au moins aussi importantes que les mesures thermiques, correspondant aux radiations à grande longueur d'onde.

Malheureusement, comme on va le voir, l'atmosphère terrestre exerce sur les radiations à courte longueur d'onde une absorption si énergique que la majeure partie du spectre solaire ultraviolet est interceptée d'une manière complète; l'étude que je m'étais proposée est donc actuellement impossible sous la forme simple que j'avais imaginée; néanmoins les résultats obtenus en vue de ces recherches présentent quelque intérêt, même au point de vue auquel je m'étais primitivement placé; ils jettent, en tous cas, un certain jour sur l'allure probable de l'absorption atmosphérique à l'autre extrémité du spectre moins facile à étudier, et montrent que le phénomène thermique est plus complexe que ne le suppose le mode expérimental généralement adopté pour l'évaluation de la température du soleil.

Pour rester dans le domaine des faits et de leurs conséquences les plus immédiates, je me bornerai ici à donner un résumé succinct des expériences à l'aide des quelles j'ai cherché à obtenir photographiquement la limite extrême ultraviolette du spectre solaire et l'analyse des conditions qui permettraient de reculer un peu cette limite.

Mode d'Observation.

Le spectroscope dont je me sers généralement est composé d'un collimateur et d'une lunette, dont les objectifs sont respectivement

formés par une lentille plan convexe de quartz (d'environ $0^m{\cdot}26$ de longueur focale pour la raie D) ; le prisme est en Spath d'Islande; on utilise le rayon ordinaire dont la dispersion est assez grande. Comme les objectifs ne sont pas achromatiques, la mise au point est variable avec la refrangibilité des rayons ; une table construite expérimentalement donne les variations de tirage de la lunette et du collimateur pour l'observation des rayons de chaque refrangibilité.

Quoique le Spath d'Islande ne soit pas aussi transparent que le quartz, néanmoins, pour l'observation du spectre solaire, l'appareil est parfaitement convenable, car l'absorption par le Spath d'Islande ne commence à devenir appréciable que pour les très courtes longueurs d'onde ($\lambda=210$ millionèmes de millimètre) ; or les dernières radiations solaires qu'on puisse observer n'atteignent pas $\lambda=290$. Des expériences comparatives faites avec des prismes de quartz et de Spath d'Islande en prenant pour source une étincelle d'induction ont montré la complète égalité de transparence des deux substances entre les limites indiquées ; l'avantage reste toutefois au Spath dont les rayons sont plus séparés et plus dispersés.

Au foyer de la lunette on place une plaque fluorescente de verre d'urane pour la vérification approchée des réglages, et finalement une petite glace sensibilisée par du collodion humide. La durée d'exposition varie, suivant les besoins, de $2\frac{1}{2}$ à 6 minutes, pour obtenir la limite extrême du spectre.

A l'aide d'un héliostat dont le miroir est remplacée par un prisme reflecteur en quartz on concentre le faisceau du lumière sur la fente du collimateur à l'aide d'une lentille de quartz. La limite du spectre est variable suivant l'état de l'atmosphère, la nature du collodion employé, et la durée d'exposition ; mais en choisissant les plus belles journées, en adoptant un collodion de composition constante, et une durée d'exposition toujours la même, on obtient des séries très comparables. Voici un exemple d'observation :—

11 Septembre, 1878. Observation fait à Courtenay (Loiret). Latitude 48° 2′ 20″. Durée d'exposition, $2^m\frac{1}{2}$—clichés renforcés une fois.

	h.	m.		
Début de l'expérience	10	30	A.M.	Limits $\lambda=295{\cdot}5$
	0	2	P.M.	295·0
	1	18	.....	295·5
	1	50	.....	297·0
	3	9	.....	299·0
	3	40	.....	302·0
	4	17	.....	304·5
	4	38	.....	307·0
	5	2	.....	312·0
	5	14	.....	315·0 ?

Remarque.—Les raies d'absorption dans l'orangé, voisines de la raie D, sont notablement plus faibles que de coutume.

La limite du spectre est exprimée en longueur d'onde, par comparaison avec la carte que j'ai construite d'après les observations faites pendant l'été de 1877.

On voit que l'étendue du spectre diminue avec la hauteur du soleil, ce qui tend à prouver que c'est à l'absorption de l'atmosphère qu'est dûe cette limitation.

Après divers essais faits d'après cette manière de voir, j'ai été conduit à représenter par la courbe suivante les résultats des observations; on porte comme abscisse la longueur d'onde, limite observée, et comme ordonnée le *logarithme du sinus de la hauteur du soleil* (on sait que les épaisseurs atmosphériques traversées par les rayons solaires sont sensiblement en raison inverse du sinus de la hauteur du soleil). L'ensemble des points ainsi définis se trouvent pour une même journée répartis sur une courbe à fort peu près rectiligne, les variations correspondant à des altérations momentanées de la transparence de l'atmosphère. Dans les belles journées les limites du spectre sont sensiblement les mêmes pour les mêmes hauteurs du soleil. Dans les journées légèrement brumeuses les limites observées sont un peu moins étendues, de sorte que le défaut de transparence se traduit par un *déplacement de la ligne parallèlement à elle-même.* La transparence de l'atmosphère pour les radiations ultraviolettes parait augmenter au mois de Septembre et Octobre, et se continuer même en hiver en Décembre et Janvier; malheureusement les belles journées sont rares en hiver, et il est difficile de décider si cette augmentation de transparence est spéciale aux temps froids, ou si elle provient de ce qu'on ne choisit, en hiver, que les journées où le ciel présente la transparence maximum.

La construction de plus d'une centaine d'observations aux diverses heures de la journée m'a conduit à adopter comme ligne moyenne la droite qui passe par le point

$$\sin h = 0{\cdot}49, \quad \lambda = 300,$$

et parallèle à la direction définie par les deux points

$$\sin h = 0{\cdot}75, \quad \lambda = 295, \qquad \sin h = 0{\cdot}30, \quad \lambda = 306$$

(relevés sur un courbe dont les ordonnées représentaient les sinus des hauteurs vraies du soleil au debut de l'observation). On en déduit l'équation empirique

$$\sin h = 0{\cdot}49\ 10^{-0{\cdot}036175(\lambda-300)},$$

ou si l'on calcule avec logarithmes supérieurs,

$$\sin h = 0{\cdot}49\ e^{-0{\cdot}08330(\lambda-300)}.$$

La limite la plus éloignée que j'ai pu obtenir correspond à la longueur d'onde 293; elle a été atteinte avec certitude deux fois seulement, le 24 Juin et le 18 Août 1878, aux environs de midi. Les radiations $\lambda=294$ à 295 ont été atteinte bien des fois de Mai à Septembre, 1877 et 1878, et récemment le 5 Mai 1879.

Malgré les nombreux essais faits dans les meilleures conditions soit à Paris, soit à la campagne, il m'a été impossible d'aller plus loin.

Examen des Causes d'Erreurs.

Cette recherche d'une limite de visibilité comporte évidemment, à un point de vue absolu, bien des causes d'erreurs, dont les deux principales sont:—

1°. Les inégalités de transparence de l'atmosphère;

2°. L'inégalité des effets photographiques provenant de la sensibilité spécifique du collodion employé et de la durée de l'exposition.

1°. *Inégalité de Transparence de l'Atmosphère.*—Cette cause d'erreur étant inévitable, on ne peut en atténuer les effets que par une longue série d'observations; il est à remarquer que si l'on prend soin de choisir des jours sans nuage ni brume, les inégalités se réduisent à bien peu de chose, et sont de l'ordre des erreurs que les accidents de la manipulation photographique peuvent introduire.

2°. *Inégalité des Effets Photographiques.*—La seconde cause d'erreur est, au point de vue pratique, bien moins grave qu'on ne pourrait le supposer au premier abord par suite de diverses circonstances que je vais brièvement rapporter.

Les collodions humides de composition diverses (iodures et bromures de cadmium et d'ammonium) ne paraissent différer que fort peu relativement à l'action qu'ils éprouvent sous l'influence des radiations très réfrangibles, en ce sens, qu'ils ne presentent pas de maximum ou de minimum pour certaines radiations,* c'est ce qu'on peut constater en photographiant des spectres d'étincelles d'induction; ces intensités relatives des raies conservent la même valeur, même bien au-delà de la région des radiations extrêmes observées dans le spectre solaire. La sensibilité plus ou moins grande se traduit seulement par la durée du temps d'exposition nécessaire pour obtenir le même effet photographique, c'est-à-dire, la même intensité du cliché.

Il ne reste donc que l'influence de la durée d'exposition qui puisse introduire des causes d'erreurs dans l'appreciation de la limite du spectre; or il se présente une circonstance extremement favorable, c'est que l'effet photographique se produit pour ainsi dire dans les première instants, de sorte que la prolongation de l'exposition n'ajoute que peu à l'étendue des radiations agissantes. C'est ce que j'ai établi

* Dans le spectre visible il n'en est pas de même notamment dans le voisinage de la raie G.

par une série méthodique d'experiences parmi lesquelles je citerai la suivant en comme typique :—

16 Oct., 1878. h. m.	Durée de l'exposition. Sur un même cliché.	Longueur de l'onde. Limite du côte ultraviolet.		
11 47 T.M.	1 seconde	λ=306		
	5	301	2·5	5·0
	20	298·5		
	100	297·5		1·0

Ainsi les durées d'exposition variant a peu près en progression géometrique, les variations de la limite perceptible diminuent très rapidement; d'après la loi que suivent les différences, pour reculer la limite seulement d'une demi-unité, il faudrait plus que quadrupler la durée d'exposition, et la porter à 500 au 600 secondes, ou 10 minutes. Aussi l'expérience a-t-elle montré qu'il était à peu près indifferent de dépasser 2 à 3 minutes pour la durée de l'exposition lorsque le temps est bien pur; lorsqu'on l'on craint le passage de brumes legères, il est plus prudent de prolonger cette durée jusques à 6 minutes afin d'augmenter les chances d'éclaircie.

La composition du collodion que j'ai toujours employé de préférence est la suivante :—

Pyroxyle.....	1 grm.
Alcool.....	40 cc.
Ether	60 cc.
Iodure de cadmium	1 grm.
Bromure de cadmium	0·25 grm.

Le révélateur est formé de—

Eau distillée	1000 cc.
Sulfate de fer.....	40 grm.
Alcool	30 cc.
Acide acétique cryst.....	30 cc.

Le bain d'argent sensibilisateur contient 8 pour cent de nitrate d'argent.

Pour renforcer le cliché on ajoute quelques gouttes d'un bain d'argent à 2 pour cent au révélateur.

Analyse des Conditions qui permettraient de reculer la Limite d'Observation.

La limitation du spectre ultraviolet paraissant due à l'absorption atmosphérique, peut-on espérer en se plaçant dans des conditions plus favorables reculer notablement la limite de visibilité? Nous allons

voir par l'analyse des conditions de l'expérience que d'après les faits observés on ne doit pas attendre une amélioration bien considérable ou du moins en rapport avec les difficultés matérielles qu'il faudrait affronter.

Exprimons, l'intensité, P, de l'impression photographique en fonction des éléments qui la déterminent. Nous admettrons que l'intensité, P, est, pour des valeurs très petites (les seules sur lesquelles nous ayions ici à raisonner) est proportionnelle à l'intensité, J_λ, de la radiade longuer d'onde, λ, et à une fonction particulière de λ et du temps, t.

$$P = J_\lambda F(t,\lambda).$$

Cette fonction F représente la *sensibilité* de la couche impressionnable.

Si la radiation avant d'agir est transmise par une milieu d'épaisseur ϵ ayant le pouvoir de transmission a_λ pour la radiation, λ, l'intensité devient:—

$$I_\lambda = {}_\lambda J a_\lambda{}^\epsilon \quad a<1.$$

Le milieu absorbant étant limité par une surface semblement plane, la direction de transmission faisant un angle, h, avec le plan, on a

$$\epsilon = \frac{l}{\sin h},$$

l étant l'épaisseur normale du milieu absorbant supposé homogène: s'il n'est pas homogène, mais formé de couches paralleles de densité variable δ, l représente la hauteur équivalente calculée par la formule

$$l\delta_0 = \int \delta dz,$$

δ étant la densité correspondant à l'épaisseur z, et δ_0 étant la densité choisie pour l'évaluation de la *hauteur réduite*, car c'est la quantité de matière traversée et non sa répartition qui produit l'absorption.

Substituant dans la valeur de P

$$P = J_\lambda F(t,\lambda) a_\lambda^{\frac{l}{\sin h}} \quad \ldots\ldots\ldots \quad (1).$$

Il y a trois fonctions inconnues de λ, à savoir, J, F, et a_λ; la forme particulière sous laquelle elles se presentent permettent de les éliminer à l'aide de l'équation empirique fournie par les observations dont il a été question plus haut. En effet on a déterminé pour un même lieu ($l=l_1$) la loi qui lie les hauteurs h du soleil à la longueur d'onde limite λ observé, c'est-à-dire à celle qui donne l'impression photographique limite $P=w$, w est alors une constante, la même pour toutes les valeurs de λ. Prenant le logarithme des deux membres de l'équation precédent où $P=w$, $t=T$ durée constante de l'exposition

$$\log w = \log J_\lambda + \log F(T,\lambda) + \frac{l}{\sin h} \log a_\lambda,$$

qui se réduit à la forme

$$\frac{\sin h}{l}=\psi(w,\mathrm{T},\lambda),$$

puisque J_λ est une fonction de λ (J_λ représente la variation de l'intensité des radiations avec la longueur d'onde du spectre continu ideal de la photosphere).

Mais l'expérience a montré que dans le même lieu ($l=l_1$) pour une durée de pose constante (T=const.), la limite d'impression (w=const.) était donnée par la loi

$$\sin h=0{\cdot}49\ e^{-0{\cdot}08330(\lambda-300)},$$

ou de la forme

$$\sin h=\mathrm{M}e^{-m(\lambda-\lambda_0)}.$$

Substituant cette valeur de sin h et de l, il vient

$$\psi(w_1\mathrm{T}_1\lambda)=\frac{\mathrm{M}e^{-m(\lambda-\lambda_0)}}{l_1},$$

de soit que la formule générale devient

$$\frac{\sin h}{l}=\frac{\mathrm{M}e^{-(\lambda-\lambda_0)}}{l_1} \quad \ldots\ldots\ldots \quad (2).$$

Si l'on se reporte à la formule primitive (1), on voit que l'absorption est d'autant moindre que la hauteur solaire est plus grande, et que l'épaisseur l est plus petite. On peut donc espérer en diminuant l'épaisseur atmosphèrique c'est-à-dire en augmentant l'altitude, z, du lieu d'observation etendre la limite de visibilité du spectre.

Le résultat précedent donne précisement la loi chérchée, c'est-à-dire l'étendue gagnée avec l'altitude. En effet, la quantité l est à une constante près representée par le poids d'une colonne atmosphèrique ayant l'unité de base comme section ; si donc on désigne par p la pression barometrique, ou aura

$$l=\mathrm{A}p.$$

Or la loi de décroissance de la pression avec l'altitude est donné par une expression de la forme

$$p=p_0e^{-\frac{z}{z_0}}.$$

z_0 etant la constante baromètrique $18336^m\times0{\cdot}43429$. Il vient finalement

$$\sin h=\mathrm{M}e^{-m(\lambda-\lambda)-\left(\frac{z-z_1}{z_0}\right)},$$

formule qui donne la loi approché qui existe entre la longueur d'onde de la radiation du spectre solaire à la limite de visibilité pour une hauteur donnée, h, du soleil, et l'altitude, z_1, du lieu d'observation.

Nous pouvons maintenant savoir ce qu'on gagne par l'accroissement

de l'altitude à hauteur égale de soleil, il suffit d'écrire que le second membre est constant, ce qui donne la condition

$$+m(\lambda-\lambda_0)+\frac{z-z_1}{z_0}=\text{const},$$

ou

$$m\lambda+\frac{z}{z_0}+\text{const}=0,$$

la longueur d'onde de la radiation limite et l'altitude sont liées par une fonction linéaire ; de sorte que leurs variations finies ou infiniment petites sont proportionnelles le co-efficient de proportionnalité est $-mz_0$.

$$dz=-mz_0d\lambda, \qquad dz=-663{\cdot}3^m d\lambda,$$

en substituant $z_0=7963^m$ et $m=0{\cdot}0833$.

Ainsi on recule la limite de visibilité des radiations d'une quantité correspondant à une unité (millionème de millimètre) dans la longueur d'onde lorsqu'on s'éleve 663·3 en altitude.

Ce gain est, relativement à la longueur du spectre que donnent les étincelles d'induction ou l'arc electrique, extrémement faible. Si l'on songe que l'on ne peut qu'en faire d'observations régulières au dessus de 4,000 mètres d'altitude, on voit que le gain calculé n'atteint que 6 unités ou environ la moitié de la différence qui se presente de l'hiver à l'été, le résultat obtenu serait donc encore bien loin de correspondre aux efforts à dépenser pour aller installer des appareils à une altitude aussi grande.

Il est vrai que l'on aurait quelque espoir d'aller un peu plus loin que la formule ne l'indique, car les co-efficients numeriques sont empruntés à des experiences faites à de faibles altitudes (au dessous de 200^m), c'est-à-dire dans les parties basses de l'atmosphère dont le pouvoir de transmission spécifique est probablement plus faible que celui des hautes regions à cause des brumes et poussières qui sont toujours en suspension.

On pourrait aussi espérer que la formule empirique fondée sur l'observation d'une très petite étendue du spectre, et employée ici *par extrapolation*, ne donne des résultats erronés dans un sens défavorable en dehors des limites où elle à été établie ; quoi qu'il soit impossible de rien affirmer avec certitude sous ce rapport, il n'est pas sans intérêt de signaler un fait qui parait donner une grande probabilité sinon à la valeur exacte des coefficients du moins à l'allure de la loi empirique proposée.

En effet, poussons l'extrapolation à l'extrême et cherchons, puisque le pouvoir absorbant de l'atmosphère parait grandir d'une manière aussi rapide avec la refrangibilité des radiations, quelle est la longueur d'onde de la radiation (ayant une intensité comparable à la radiation correspondante du soleil) qui donnerait la limite d'impression photo-

graphique après avoir traversé un mètre d'atmosphère à $0^{m}{\cdot}760$ de pression. C'est à peu près la *longueur réduite* d'un spectroscope: c'est donc la limite possible d'étendue des spectres observables au spectroscope, les radiations plus réfrangibles étant absorbées par l'atmosphère et les appareils.

Nous prendrons la formule (2) sous la forme—

$$\frac{l_1}{l} \sin h = \mathrm{M} e^{-m(\lambda-\lambda)},$$

le rapport $\frac{l_1}{l}$, represente le rapport de épaisseurs réduites ou le rapport des pressions correspondant à ces épaisseurs: on peut adopter ici $\frac{l_1}{l} = \frac{1}{7600}$, puisque l'élévation de 1m correspond à peu près à un dixième de millimètre dabaissement du baromètre: on posera $\sin h = 1$, la transmission étant normale et l'on aura a résoudre l'equation numerique

$$\frac{1}{7600} = 0{\cdot}49\, e^{-0{\cdot}48330(\lambda-300)},$$

ou

$$= 0{\cdot}49\, 10^{-0{\cdot}046175(\lambda-300)},$$

on trouve $\lambda = 184{\cdot}2$.

Comme on a pris un peu arbitrairement 1 metre on pourrait croire qu'en choisissant une autre valeur la limite changerait beaucoup; il n'en est rien la limite change extrememement peu, car en diminuant l'épaisseur traversée l_1 de moitie on trouve $\lambda = 173$, la longueur reduite d'un spectroscope est evidemment comprise entre ces limites.

Or lorsqu'on examine un spectroscope construit tout en quartz ou tout en Spath fluor, par fluorescence, ou par photographie, les spectres des étincelles d'induction, en leur donnant toute l'energie que donnent une forte pile et une bobine de grande dimension, on n'arrive guère à depasser la réfrangibilité de la raie No. 32 de l'aluminium dont la longueur d'onde est 184 environ, ainsi que je l'ai mesurée directement à l'aide d'un réseau.

Je me suis assuré en outre qu'en plaçant l'etincelle à 4 mètres de l'appareil, dans des circonstances telles que l'éclat des raies moins réfrangibles No. 30 et No. 31 (notations de M. Soret) soit à peine diminué, la raie 32 disparait. On ne peut donc guère attribuer ce fait qu'à l'absorption atmosphèrique.

Donc la formule empirique déterminée plus haut, ne doit pas être très erronée lorsqu'on l'emploie par extrapolation dans le voisinage des limites où elle a été établie; par conséquent les chances d'extension de nos connaissances sur la partie ultraviolette du spectre solaire avec l'altitude au de là de ce qu'indique cette formule ne sont pas très considérables.

III. "On an Induction-Currents Balance, and Experimental Researches made therewith." By Professor D. E. HUGHES. Communicated by Professor HUXLEY, Sec. R.S. Received May 5, 1879.

Immediately upon the announcement of Arago's discovery of the influence of rotating plates of metal upon a magnetic needle (1824), and Faraday's important discovery of voltaic and magneto-induction (1831), it became evident that the induced currents, circulating in a metallic mass, might be so acted upon either by voltaic or induced currents as to bring some new light to bear on the molecular construction of metallic bodies.

The question was particularly studied by Babbage, Sir John Herschel, and by M. Dove* who constructed an induction balance, wherein two separate induction coils, each having its primary and secondary coils, were joined together in such a manner that the induced current in one coil was made to neutralize the induced current in the opposite coil, thus forming an induction balance, to which he gave the name of differential inductor. In those days physicists did not possess the exquisitely sensitive galvanometers and other means of research that we possess at the present day, but sufficiently important results were obtained to prove that a vast field of research would be opened if a perfect induction balance could be found, together with a means of correctly estimating the results obtained. In experimenting with the microphone I had ample occasion to appreciate the exquisite sensitiveness of the telephone to minute induced currents. This led me to study the question of induction by aid of the telephone and microphone. The results of those researches have been already published.†

Continuing this line of inquiry, I thought I might again attempt to investigate the molecular construction of metals and alloys, and with this object I have obtained, after numerous comparative failures, a perfect induction balance which is not only exquisitely sensitive and exact, but allows us to obtain direct comparative measures of the force or disturbances produced by the introduction of any metal or conductor.

The instrument which I have the honour to present to the Royal Society this evening, consists, 1st, of the new induction-currents balance; 2nd, microphone, with a clock as a source of sound; 3rd, electric sonometer, or absolute sound measurer, a late invention of my own; 4th, a receiving telephone and three elements of Daniells' battery.

* De la Rive, "Treatise on Electricity," vol. i, chap. v. London, 1853.

† "Comptes Rendus," December 30, 1878, and January 20, 1879; Society of Telegraph Engineers, March 12, 1879.

In order to have a perfect induction-currents balance suitable for physical research, all its coils as well as the size and amount of wire should be equal. The primary and secondary coils should be separate and not superposed. The exterior diameter of the coils presented this evening is $5\frac{1}{2}$ centims., having an interior vacant circular space of $3\frac{1}{2}$ centims.; the depth of this flat coil or spool is 7 millims.

Upon this box-wood spool are wound 100 metres of No. 32 silk-covered copper wire. I use four of such coils, formed into two pairs, the secondary coil being fixed permanently, or by means of an adjustable slide, at a distance of 5 millims. from its primary; on the second similar pair there is a fine micrometer screw, allowing me to adjust the balance to the degree of perfection required.

These two pair of coils should be placed at a distance not less than 1 metre from each other, so that no disturbing cause should exist from their proximity.

The two primary coils are joined in series to the battery, the circuit also passing through the microphone.

In place of the telephone I have sometimes used a magnetic pendulum, the swing of the arc described indicating and measuring the forces.* I am at present engaged upon a very sensitive voltameter which shall indicate and measure the force of rapid induced currents. The telephone, however, is well adapted as an indicator, but not as a measurer of the forces brought into action. For this reason I have joined to this instrument an instrument to which I have given the name of electric sonometer. This consists of three coils similar to those already described, two of which are placed horizontally at a fixed distance of 40 centims. apart, and the communication with the battery is so arranged that there are similar but opposing poles in each coil; between these there is a coil which can be moved on a marked sliding scale divided into millimetres; in a line with these two opposing primary coils, the centre coil is the secondary one, and connected by means of a circuit changing key with the telephone in place of the induction balance. If this secondary coil is near either primary coil, we hear loud tones, due to its proximity. The same effect takes place if the secondary coil is near the opposing coil, except that the induced current is now in a contrary direction, as a similar pole of the primary acts now on the opposite side of the induction coil; the consequence is, that as we withdraw it from one coil approaching the other, we must pass a line of absolute zero, where no current whatever can be induced, owing to the absolutely equal forces acting equally on both sides of the induction coil. This point is in the exact centre between the two coils, no matter how near or distant they may be. We thus possess a sonometer having an absolute zero of sound; each degree that it is moved is accompanied by

* "Telegraphic Journal," December 15, 1878.

its relative degree of increase; and this measure may be expressed in the degrees of the millimetres passed through, or by the square of the distances in accordance with the curve of electro-magnetic action.

If we place in the coils of the induction balance a piece of metal, say copper, bismuth, or iron, we at once produce a disturbance of the balance, and it will give out sounds more or less intense on the telephone according to the mass, or if of similar sizes, according to the molecular structure of the metal. The volume and intensity of sound is invariably the same for a similar metal. If by means of the switching key the telephone is instantly transferred to the sonometer, and if its coil be at zero, we should hear sounds when key is up or in connection with the induction balance, and no sounds or silence when the key is down or in connexion with the sonometer. If the sonometer coil was moved through several degrees, or through more than the required amount, we should find that the sounds increase when the key is depressed; but when the coil is moved to a degree where there is absolute equality, if key is up or down, then the degree on scale should give the true value of the disturbance produced in the induction balance; and this is so exact that if we put, say a silver coin whose value is 115°, no other degree will produce equality. Once knowing, therefore, the value of any metal or alloy, it is not necessary to know in advance what the metal is, for if its equality is 115°, it is silver coin; if 52, iron; if 40, lead; if 10, bismuth; and as there is a very wide limit between each metal, the reading of the value of each is very rapid, a few seconds sufficing to give the exact sound value of any metal or alloy.

During the course of these experiments with this instrument I noticed that my own hearing powers varied very much with state of health, weather, &c., that different individuals had wide differences of hearing, and that nearly in all cases one ear was more sensitive than the other; thus whilst my degree of hearing was 10°, another might be 60 in one ear and 15 in the other.*

Dr. Richardson, F.R.S., who upon my invitation investigated this subject, became so impressed with the value of the instrument as an absolute measure of our hearing powers, and its capabilities of throwing much light upon its relation with health, that he has undertaken a series of researches which will extend over some time, and which I think from some facts already gained will be of great value to the medical profession. These experiments are now in his very able hands, and he will in due time announce the results to the Royal Society.

If an observer's hearing is limited to 10°, how can we hear results below this line? I should have stated that when used to measure the

* To this portion of my instrument when used as a measurer of our hearing powers, we have given the name of audiometer.

hearing power, we determine on a constant standard of force such as one element Daniell, but if we increase the number of elements we in same ratio increase the inductive disturbance, and thus by a large increase of force bring within our range results too feeble to be heard without its aid, the sonometer constantly, however, giving the same degree for equality as the increased force is also used on this instrument. Thus in our measurements we can entirely neglect the amount of battery, as its comparative results remain a constant.

As a rule three Daniell elements will be found quite sufficient, and even this weak force is so exquisitely sensitive that it will find out the smallest fraction of difference in weight or structure of metals. Thus two silver coins such as a shilling, both quite new, and both apparently of the same weight, will be found to possess a difference of weight which the instrument at once indicates.

The following experiments will show its exceeding sensitiveness and its wide field of usefulness as an instrument of research.

I. If we introduce into one pair of the induction coils any conducting body, such as silver, copper, iron, &c., there are set up in these bodies electric currents which react both upon the primary and secondary coils, producing extra currents whose force will be proportional to the mass, and to its specific conducting powers. A miligramme of copper on a fine iron wire, finer than the human hair, can be loudly heard and appreciated by direct measurement, and its exact value ascertained. We can thus weigh to an almost infinitesimal degree the mass of the metal under examination; for instance, if we take two English shilling pieces fresh from the Mint, and if they are absolutely identical in form, weight, and material, they will be completely balanced by placing one each in the two separate coils, provided that for these experiments there is an adjustable resting place in each pair of coils, so that each coin may lie exactly in the centre of the vacant space between the primary and secondary coils. If, however, these shillings are in the slightest degree worn, or have a different temperature, we at once perceive this difference, and if desired, measure it by the sonometer, or, by lifting the supposed heaviest coin at a slight distance from the fixed centre line, the amount of degrees that the heaviest coin is withdrawn will show its relative mass or weight as compared with the lightest. I have thus been able to appreciate the difference caused by simply rubbing the shilling between the fingers, or the difference of temperature by simply breathing near the coils, and in order to reduce this sensibility within reasonable limits, I have only used in the following experiments 100 metres of copper wire to each coil and 3 cells of battery.

II. The comparative disturbing value of disks of different metals, all of the same size and form of an English shilling, and measured in millimetre degrees, by the sonometer, is the following:—

Silver (chemically pure)	125	Iron (chemically pure).....	45
Gold „ „	117	Copper (antimony alloy)	40
Silver (coin)	115	Lead	38
Aluminium	112	Antimony	35
Copper.....	100	Mercury	30
Zinc.....	80	Sulphur (iron alloy)	20
Bronze.....	76	Bismuth	10
Tin.....	74	Zinc (antimony alloy)	6
Iron (ordinary)	52	Spongy gold (pure)	3
German silver.....	50	Carbon (gas)	2

These numbers do not agree entirely with any lists of electrical conductivity I have yet met with; the numbers are, however, invariably given by the sonometer, and the divergence may be due to some peculiarity of structure of the metals when formed into disks. Future investigations with this instrument will, no doubt, give more correct values than I have been able to obtain with my limited means of research.

III. It will be seen from the above, that the instrument gives very different values for different metals or alloys, consequently, we cannot obtain a balance by employing two disks of different metals, and the instrument is so sensitive to any variation in mass or matter, that it instantly detects the difference by clear loud tones on the telephone. If I place two gold sovereigns of equal weight and value, one in each coil, there is complete silence, indicating identity or equality between them; but if one of them is a false sovereign, or even gold of a different alloy, the fact is instantly detected by the electrical balance being disturbed. The instrument thus becomes a rapid and perfect coin detector, and can assay any alloy, giving instantly its electrical value. The exceeding sensitiveness of this electrical test I shall demonstrate by experiment this evening. Again, as regards coins, it resolves an almost magical problem. Thus, if a person puts one or several coins into one pair of coils, the amount or nominal value being unknown to myself, I have only to introduce into the opposite coils, different coins successively, as I should weights in a scale, and when perfect balance is announced by the silence, the amount in one box will not only be the same nominal value but of the same kind of coin.

IV. We find by direct experiment with this instrument, that the preceding results are due to electric currents, induced by the primary coil, and that it is by the reaction of these that the balance is destroyed, for, if we take an insulated spiral disk or helix of copper wire, with its terminal wires open, there is no disturbance of the balance whatever, notwithstanding that we have introduced a comparatively large amount of copper wire; but on closing the circuit, the balance is at once very powerfully disturbed.

If the spiral is a flat one, resembling a disk of metal, and circuit closed, we find that loud tones result when the spiral is placed flat, or

when its wire is parallel to those on coils but if it is held at right angles to these wires, no sound whatever is heard, and the balance remains perfect. The same thing occurs with disks of all non-magnetic metals, and a disk of metal placed perpendicular to the coils exerts no influence whatever. The contrary result takes place with a spiral of iron wire or disk of iron; the induced current circulating in the spiral is at its maximum when the spiral lies flat or parallel with the coils, being reduced to nothing when at right angles, but the disturbance of the induction balance is more than four times as great when the spiral is perpendicular to the wires of the coils as when parallel with the same. That this result is simply due to the property of magnetic bodies of conduction of magnetism, we shall see in some following experiments.

That the currents in non-magnetic metals travel in a circle corresponding to that of the primary coil, may be seen with spongy gold. In its first extremely divided state it falls below our zero of hearing, on slightly shaking the bottle we have 2° as its value, on pressing it its value rapidly increases with the pressure, until when formed into a solid disk its value becomes 117°.

V. The instrument proves, that a very remarkable difference exists in bars of iron of the same exact form and size, but of different origin or treated in a different manner; in point of fact, no two bars, cut off of the same rod, and treated alike, are exactly of the same value, or induce a complete balance.

Mr. Stroh, the eminent instrument maker, has kindly furnished me with numerous samples, varying in value in degrees of the sonometer from 100 to 160.

Chemically pure iron was found to be the best, but still very slightly superior to ordinary iron, which had been drawn into a wire of the required thickness. The fibrous condition thus developed is highly favourable (if the iron is softened by heat) for the conduction of magnetism. From numerous examples I select a few indicative values:—

	Softened.		Tempered.
Chemically pure iron.....	160	.....	130
Forged soft iron.....	150	.....	125
Wire-drawn iron.....	156	.....	120
Cast steel.....	120	.....	100

VI. As yet the instrument has given no indications of molecular change produced by magnetism in non-magnetic bodies, but the great change which takes place in all magnetic bodies, except hard-tempered cast steel, indicates that a molecular change of structure, analogous to that of tempering, takes place upon iron, steel, and nickel.

If we place a disk of iron in one of the coils, we find that the balance is destroyed, and that the iron has weakened the induction by

the absorption of work done in inducing the circular currents. This can be perfectly balanced by placing a small coin or disk of silver or copper in opposite coils; but if an iron wire or rod is placed perpendicular to the coils, then increase of inductive force takes place in those coils by the conduction of induced magnetism from primary to secondary, and the iron can no longer be balanced by silver, copper, or any non-magnetic metal. The coils must be either removed farther apart, so as to reduce the increased force, or balanced by an equivalent amount of iron or magnetic conduction in opposite coils.

An interesting case of both reduction and increase of force in the same pair of coils occurs if we place a disk of iron, not in the centre of coils, but in the vacant space between the coils. We thus reduce the force by 150°. If, in addition to this, we place iron wires perpendicular and in the centre, there is increase of force, and if this increase is so proportioned as to be 150°, we immediately restore the balance, and we have here in the same coil two separate pieces of iron, each disturbing the balance and giving out loud tones, but producing no effect whatever, when both are introduced at the same time, complete silence being the result.

VII. These coils prove what has already been long known, viz., that hard steel has a far less conducting power for magnetism than soft iron, although the hard steel has a far higher retaining power. This instrument demonstrates a point, which I have not yet seen remarked, that magnetism does not in itself change the conducting power, but that it produces a molecular change of structure in iron, analogous to that of tempering; for if we balance two soft iron rods against each other, the balance being made perfect by the addition of fine iron wires on the weakest side, we find that on strongly magnetising this bar, by drawing it across a strong compound magnet, and on replacing it in its coil, it has lost 30 per cent. of its conducting power; or if, instead of magnetising, we make this iron red hot and plunge it in cold water, the loss of conducting power will be very similar—25 to 30°. If these experiments are repeated upon various degrees of iron approaching steel in character, we find that as it already possesses hardness or temper, it is less and less affected by magnetism, until we arrive at hard cast steel, where magnetism no longer produces any change in its conducting power. From this I draw the conclusion that the effect of magnetism is very similar to that of temper, and shall show, under the effects of strain and torsion, that magnetism produces this temper or strain perpendicular to the lines of magnetic force.

VIII. The instrument shows that a remarkable change takes place in the magnetic conducting power of iron and steel on subjecting the wire under examination to a longitudinal strain; for if we pass an iron wire through the centre of both coils, half a millimetre diameter and

20 centims. or more in length, so arranged by a winding key that we can apply a strain to this wire, we find a magnetic conducting value, unstrained, of 100, but on applying a slight strain its value rapidly increases, being more than double at its breaking point. If during this strain we strike the wire, we hear its musical tone, and no matter how much we may wind or unwind it, provided we do not pass its limits of elasticity and similar wire is used, the same musical tone will invariably give the same magnetic value. Thus the note A, or 435 complete vibrations per second, gave always the magnetic value of 160, or 60 per cent. increase of power over the unstrained wire. If whilst this wire is strained, giving the value 160, we magnetise it by drawing over it a strong compound magnet, the note remains the same, showing no difference of tension, but its magnetic value has fallen 80°, being now 80 instead of 160; and this wire can never again be brought by strain up to its previous high conducting powers. Now as we have seen that magnetism produces no change in hard tempered steel, but that in soft iron it produces a change very analogous to that of temper; and as the effect of strain would be also to harden the fibres by bringing them all parallel to the line of mechanical strain, and as this improves its conducting power, while magnetism instantly destroys all the benefits of the longitudinal mechanical strain, we can only draw the conclusion that magnetism produces a strain analogous to temper, but contrary to that of the longitudinal mechanical strain; in other words, that the magnetic strain is produced perpendicularly to its lines of force.

This view is sustained by the effects of torsion; for if, in place of straining the wire, it is twisted, instead of increasing, it rapidly decreases in magnetic conductive value, each turn or twist diminishing its power of conduction in a remarkably constant line of decrease. At 80 turns of this wire there was a decrease of 65 per cent.; at 85 turns the wire broke, and on testing it to see if magnetism had any decreasing effect on it, I found that it produced no change whatever; but this twisted soft iron wire had now remarkable permanent retaining powers of magnetism, being superior to tempered cast steel.

Again, if we take three similar pieces of soft iron wire, leave the first for comparison in its natural condition, strain the second by a longitudinal strain until it is broken, and twist the third by a torsion-key until it also is broken; we find on magnetising equally these three wires, and allowing ten minutes' repose, that the first or untouched wire has a retaining power of magnetism of 100, the second only of 80, and the third, or twisted wire, of 300. I hope, by the light thus given, soon to be able to produce a magnet whose force shall be greatly in excess of what we have hitherto possessed; our difficulty at present being that in order to temper steel, we must heat it to redness, and this allows the molecules to rearrange themselves contrary to the object we have in view.

IX. There is a marked difference of the rapidity of action between all metals, silver having an intense rapidity of action. The induced currents from hard steel, or from iron strongly magnetised, are much more rapid than those from pure soft iron; the tones are at once recognised, the iron giving out a dull, heavy smothered tone, whilst hard steel has tones exceedingly sharp. If we desire to balance iron, we can only balance it by a solid mass equal to the iron to be balanced. No amount of fine wires of iron can balance this mass, as the time of discharge of these wires is much quicker than that of a larger mass of iron. Hard steel, however, can be easily balanced not only by steel but by fine iron wires, and the degree of the fineness of these wires required to produce a balance gives a very fair estimate of the proportionate time of discharge. The rapidity of discharge has no direct relation with the electrical conductivity of the metal, for copper is much slower than zinc, and they are both superior to iron.

X. The instrument shows a marked difference in all metals, if subjected to different temperatures. The value is reduced in non-magnetic metals, and this we should expect from the known influence of temperature on the electrical conductivity; but in the case of iron, steel, and nickel (as it has already been remarked by many), the contrary takes place, namely, a far higher degree of magnetic conductivity. A bar of soft iron, whose value at the temperature of the room, 20° C., was 160, became on heating it to 200° C. 300, that is to say, its value was nearly doubled. A bar of pure nickel, whose value at 20° was 150, became on heating it to 200°, 320; thus, in the case of nickel, its value for magnetic conductivity was more than doubled, and at this heat it surpassed the chemically pure iron at the same heat, giving a magnetic value of 320 against 300 for the iron, but at the normal temperature of 20° the iron had more magnetic power of conduction than nickel. Heating nickel, by simply plunging it into boiling water, increased its force from 150 to 250; plunging this same bar into ordinary cold water reduced its value to 130; thus the mere difference of the normal temperature of the air in the room and water which had been in this room some hours produced 20° of difference. In fact, I found that the radiant heat from the hand would raise the magnetic value several degrees, and thus nickel may be regarded as a magnetic thermometer far more sensitive than the ordinary mercurial Centigrade.

The instrument also measures the electrical resistance of wires or fluids. In order to make it do this, we have only to place the resistance to be measured across the two wires of one induction coil and on the other known resistance units. In this way we can produce a perfect balance, for it then becomes an induction bridge, the results and modes of testing of which are somewhat similar to Wheatstone's bridge.

It measures also the electrostatic capacity of Leyden jars or condensers, and is sufficiently sensitive to appreciate and measure a surface of tinfoil not larger than 4 inches square, the condenser being simply placed between the wires of one pair of coils, and the disturbance produced being measured on the sonometer.

I could cite many more interesting experiments in other branches of physical research for which this instrument offers a wide field of observation; but my object this evening is neither to broach new theories nor to correlate at present the results obtained with views already advanced by Ampère and others.

My only desire has been and is to show the wide field of research the instrument opens to physical inquirers. I trust that in more able hands it may serve to elucidate many physical phenomena.

IV. "Some Researches with Professor Hughes' new Instrument for the Measurement of Hearing; the Audiometer." By Benjamin Ward Richardson, M.D., LL.D., F.R.S. Received May 14, 1879.

Professor Hughes having done me the honour to show me first his newly invented instrument for the measurement of hearing, and having supplied me with an instrument for the purpose of testing the application of it for physiological and practical purposes, I have been enabled to make a considerable number of experiments, on which I venture to submit the following preliminary report:—

The instrument, as it has been used, is before the Society. It consists of two Leclanché's cells for the battery, a new and simple microphonic key connected with the cells and with two fixed primary coils, and a secondary or induction coil the terminals of which are attached to a telephone. The induction coil moves on a bar between the two fixed coils, and the bar is graduated into 200 parts, by which the readings of sound are taken. The graduated scale is divided into 20 centims., and each of these parts is subdivided into 10, so that the hearing may be tested from the maximum of 200 units to 0°—zero. The fixed coil on the right hand contains 6 metres of wire; the fixed coil on the left hand contains 100 metres. By this means a long scale from the left hand coil is produced. The secondary coil contains 100 metres of wire.

In using the instrument, one Leclanché's cell has been found sufficient, as a general rule, but two have been used in instances where the hearing of the person under test has been very defective. The Leclanché cell was selected by Professor Hughes as affording a reliable current for the purposes he had in view, and for standard comparisons.

In using the instrument, the induction coil is moved along the scale from or towards the larger primary, as may be required, and the degrees or units of sound are read from the figures on the scale, the sound being made by the movement of the microphonic key between the battery and the primary coils.

The person whose hearing is being tested should sit in an easy position, and should not see the act of the observer in moving the microphone key. For good observation, the room in which the experiment is made should be large, and all external causes of sound, such as the ticking of clocks, the vibrations of windows and doors, the moving of feet and the singing from gas jets, should be silenced. The sitter should close the ear that is not applied to the telephone while he is listening for minute sounds, and should give his full and calm attention to the proceeding. Any excitement is like to lead to error when refined measurements are required.

Capacity of the Instrument for Observation.

The instrument may be considered to afford the most satisfactory means for testing the hearing power of all persons who can define a sound. The range of sound is sufficient at the maximum—200°—for everyone who is not absolutely deaf; 0°, or zero, is a point of positive silence from the instrument, or rather from the sound which it produces through the telephone.

Phenomena from Observations made on different Persons.

Abrupt Loss of Sound.

One of the first facts learned with the audiometer is the suddenness with which the sound is lost to those who are listening. The sound is abruptly lost within a range of 2°; that is, within one-hundredth part of the entire scale. This is the case with those who are very deaf as well as with those who hear readily, a fact originally noticed by Mr. Hughes, and which I have corroborated by fifty special observations on different persons presenting powers of hearing which varied from 200° to 140° as the extreme limit or capacity, to complete hearing through the whole scale, down to zero. In these persons, when the observation was taken, under the strictest possible conditions for surrounding silence, the point between distinctness of sound and complete loss of it was not more than one-hundredth part of the scale.

Continuous Hearing in Line.

In testing the capacity of hearing, it is noticeable that the power to detect the diminishing sound is maintained best by continuing the reduction in trace or line while the attention is fixed. A sudden break may cause the sound to be lost to the listener long before his real inca-

pacity to hear is reached. If, for instance, the sound be very faintly heard at 15°, and the induction coil be suddenly moved to 5°, the sound at 5° may be quite inaudible; but if the coil be slowly moved, unit by unit, from 15° to 5°, the sound at 5° may be distinctly heard. Mr. Maitland Tate, C.E., who noticed this point very markedly in his own case, when I was submitting him to test, compared this to what is observed by the sense of sight in making surveys. The eye will follow a line to an extreme point with comparative readiness, but if it break away from it, the object seems to have disappeared.

Influence of Respiration on Hearing.

The effect of filling the chest and holding the breath makes a difference in listeners. The capacity for hearing is for a few seconds increased by holding the breath. Mr. Tate, who could hear with his right ear only down to 8° under ordinary breathing, could hear down to 5° when he held his breath. Another gentleman, who could hear only down to 100° under ordinary breathing, could hear to 80° when the breath was held with the chest full. Holding the breath with the chest not full fails to produce the same result.

Influence of habitual Movements of the Body.

As a rule, the hearing of persons who are right-handed is most refined in the right ear, and as most persons are right-handed, it is found that the right ear is the best ear. This rule is, however, attended with many exceptions, since, for various reasons, some persons who use the right hand exclusively, practise for some particular purpose the use of the left ear, upon which that ear becomes more acute. Thus five physicians, who were right-handed but who had accustomed themselves to use the stethoscope with the left ear, could hear to zero on that side, but had lost from 4° to 5° on the right side. Four other persons who were similarly circumstanced were able at once to account for the fact by the habit they had acquired of listening to a public discourse or sermon from the left side. Another point of interest attaching to this observation is, that the practice of using one ear for special refinement of the sense seems for the time slightly to impair the other ear, although there is no physical evidence of such impairment.

Influence of some Automatic Adjustment and of Memory, on Hearing.

Connected with the last-named fact is another, namely, that by this instrument the deaf are found to fail in capacity of hearing not only by reason of physical defect, but also by failure of memory of sounds. Thus in a youth who had suffered serious defect of hearing for seven years owing to partial destruction of the tympanum, and who in the right ear could only detect sound at 107°, there was an inability to

catch all the sound lying between 130° and 107°, until he could remember what he had to listen for. By practising him then to detect the lowest sound that he was physically capable of receiving, I got him to detect this one sound more readily than those which came higher up. By further practice all the intervening sounds became audible with equal facility. These facts, which have been confirmed by another observation on a different person, seem to me to indicate that deafness from imperfection of the tympanum or other parts of the organ of hearing may be increased, beyond the mere physical failure, either from some lost power of automatic adjustment in the auditory apparatus, or from failure of receptive power in the cerebrum itself, so that the memory, rendered imperfect, is slow to assist the listener until by exercise of function its readiness is restored.

Influence of Atmospheric Pressure.

By use of the audiometer, the influence of atmospheric pressure on hearing is detectable. In my own case when the barometer is at 30 I can hear on both sides close down to zero; but below 30 I fail by 2° on the left side to reach zero. In another person a similar failure extends to a loss of 4°.

Observations on Lower Animals.

I have tried to determine in some of the lower animals whether there is the same sense of hearing as in man. In most animals it is difficult to obtain sufficient quietude to enable the observer to gather from expression or movement of the animal the information sought for. In two dogs, one a terrier, the other a field spaniel, I succeeded in making some good observations, and in them the range of hearing power seemed to be distinctly lower than it is in the human subject who has perfect hearing. In both these animals, which were healthy, and in the prime of life, the first indication of the detection of sound commenced at 10° on the scale. The detection was evidenced by the sudden expression of listening, by a slight change of position, and a slight dilatation of the pupils. This detection was clearly made on the instant, as if the sharp line of hearing were the same in them as it is in the higher animal.

PRACTICAL MEMORANDA.

1. The audiometer will, I think, be an essential in all physical examinations of men who are undergoing examination as to their fitness for special services requiring perfect hearing, such as soldiers, sentries, railway officials, and the like.

2. The instrument will be of great use to the physician in determin-

ing the value of hearing in those who are deaf, and in determining the relative values of the two organs of hearing. In one instance, already, I have been able by its means to detect in a person who was supposed to be equally deaf on both sides, that on one side the hearing is perfect close up to zero, while on the other side nine-tenths of the hearing is lost.

3. In other forms of diagnosis I have found the instrument useful. In a young person suffering from acute anæmia the hearing was so defective that on the right side it failed to detect sound at 18°, and on the left side at 15°. In ten days, during which, under a new regimen, great improvement took place in strength and general condition, the power of hearing had so much improved that the right ear was good down to 12°, and the left to 3°; an improvement of 6° on the right, and of 12° on the left side.

In another person who was subject to repeated vertigo, the giddiness occurring three or four times a-day, the hearing was so defective that although the external ear on each side was clear and the tympanum natural, no sound could be heard below 30°. Under complete rest and attention to diet, the vertiginous attacks were in a few days removed altogether, and with that removal there was gain of hearing, on both sides equally, up to 5° on the audiometric scale.

4. The instrument may be used to differentiate between deafness through the external ear and deafness from closure of the Eustachian tube,—throat deafness. In my own case I fail to detect sound by the mouth at 170°, and this I find is a fair average in those who are healthy. It represents the comparative value of communication by sound through the Eustachian canal and the external ear.

5. The instrument promises to be very useful in detecting the effects in the body of those agents which quicken or excite the circulation, such as alcohol and other similar chemical substances. I have some observations on these points in reserve for a further report.

6. The instrument promises to be of great service in determining the value of artificial tympanums in instances of deafness due to imperfection or destruction of the natural tympanum. The cotton artificial tympanums introduced originally by the late Dr. Yearsley, and the membranous tympanums introduced by the late J. Toynbee, F.R.S., have proved of much service; and, by means of the audiometer, I have been able very accurately to test their respective merits and to compare both with tympanums made of other material. The inquiry has led me to test different metals for this purpose, and to find in fine gold the substance for making the most useful and effective artificial drum. The Messrs. Ash, of Broad Street, have made for me various forms of gold drums, the best of which, perhaps, is a little cylinder which, with other forms, is placed before the Society. With one of these cylinders I was able to give to a young gentleman who had lost

hearing on the right side up to 110°, from destruction of the tympanum, an instant gain down to 60°, a gain of 50° on the audiometric scale.

In this preliminary report I have omitted many subjects of interest, but I hope I have related enough to show that the world of science in general, and the world of medicine in particular, is under a deep debt of gratitude to Professor Hughes for his simple and beautiful instrument, which I have christened the audimeter, or less correctly but more euphoniously, the audiometer.

V. "Note on the Invention of a Method for making the Movements of the Pulse Audible by the Telephone. The Sphygmophone." By Benjamin Ward Richardson, M.D., LL.D., F.R.S. Received May 14, 1879.

While experimenting with the audiometer, it occurred to me that I might get a secondary or telephonic sound from the movements of the pulse at the wrist. I have effected this in a very simple manner, by adding a microphone to a Pond's sphygmograph. I mount on a slip of talc, glass, wood, or ebonite a plate of metal or gas carbon. I place the slip in the sphygmograph as if about to take a tracing of the pulse. I connect one terminal from a Leclanché's cell to the metal or carbon, and the second terminal from the cell to a terminal of the telephone. Then I connect the other terminal of the telephone with the metal rod of the sphygmograph. The instrument is now ready for use. It is placed on the pulse, in the ordinary way, and is adjusted, with the writing needle thrown back, until a good pulsating movement of the needle is secured. When the movement is in full action, the needle is thrown over to touch the platinum plate, which it traverses with each pulse-movement, and completes the connexion between the telephone and the battery. The needle, in passing over the metal or carbon plate, causes a distinct series of sounds from the telephone, which correspond with the movements of the pulse. When all is neatly adjusted, the sounds heard are three in number, one long sound and two short, corresponding to the systolic push, the arterial recoil, and the valvular check. The sounds are singular, as resembling the two words, "bother it." The sounds can be made very loud by increasing the battery power.

This little instrument is not so good a recorder of the pulse as the sphygmograph, but it may be made very useful in class, for illustrating to a large number of students, at one time, the movements of the natural pulse, and the variations which occur in disease. I call the invention the sphygmophone.

VI. "On the Capillary Phenomena of Jets." By Lord RAYLEIGH, F.R.S. Received May 5, 1879.

When water issues under high pressure from a circular orifice in a thin plate, a jet is formed whose section, though diminished in area, retains the circular form. But if the orifice be not circular, the section of the jet undergoes remarkable transformations, which were elaborately investigated by Bidone,* many years ago. The peculiarities of the orifice are exaggerated in the jet, but in an inverted manner. The following examples are taken from Bidone's memoir.

FIG. 1.



Fig. 1, orifice in the form of an ellipse (A), of which the major axis is horizontal, and 24 lines long; the minor axis is vertical, and 17 lines long. The head of water is 6 feet.

Near the orifice the sections of the vein are elliptical with major axis horizontal. The ellipticity gradually diminishes until at a distance of 30 lines from the orifice the section is circular. Beyond this point the vertical axis of the section increases, and the horizontal axis decreases, so that the vein reduces itself to a flat vertical sheet, very broad and thin. This sheet preserves its continuity to a distance of 6 feet from the orifice, where the vein is penetrated by air.

B represents the section at a distance of 30 lines from the orifice. It is a circle of 16 or 17 lines diameter.

C is the section at a distance of 6 inches from the orifice. It is an

* "Expériences sur la Forme et sur la Direction des Veines et des Courans d'Eau lancés par diverses Ouvertures." Par George Bidone.

elliptical figure, whose major axis is 22 lines long and minor axis 14 lines long.

D is the section at 24 inches from the orifice. It also is an elliptical figure, whose vertical axis is 45 lines long and horizontal axis about 12 lines long.

In fig. 2, the orifice (A) is an equilateral triangle, with sides 2 inches long. The head of water is 6 feet. The vein resolves itself into three flat sheets disposed symmetrically round the axis, the planes

Fig. 2.



of the sheets being perpendicular to the sides of the orifice. These sheets are very thin, and retain their transparence and continuity to a distance of 42 inches, reckoned from the orifice. The sections represented by B, C, D, E are taken at distances from the orifice equal respectively to 1 inch, 6 inches, 12 inches, and 24 inches.

Similarly, a vein issuing from an orifice in the form of a regular polygon, of any number of sides, resolves itself into an equal number

of thin sheets, whose planes are perpendicular to the sides of the polygon.

Bidone explains the formation of these sheets, in the main (as it appears to me), satisfactorily, by reference to simpler cases of meeting streams. Thus equal jets, moving in the same straight line, with equal and opposite velocities, flatten themselves into a disk, situated in the perpendicular plane. If the axes of the jets intersect obliquely, a sheet is formed symmetrically in the plane perpendicular to that of the impinging jets. Those portions of a jet which proceed from the outlying parts of an unsymmetrical orifice are considered to behave, in some degree, like independent meeting streams.

In many cases, more especially when the orifices are small and the heads of water low, the extension of the sheets in directions perpendicular to the jet reaches a limit. Sections taken at greater distances from the orifice show a gradual shortening of the sheets, until a compact form is attained, similar to that at the first contraction. Beyond this point, if the jet retains its coherence, sheets are gradually thrown out again, but in directions bisecting the angles between the directions of the former sheets. These sheets may, in their turn, reach a limit of development, again contract, and so on. The forms assumed in the case of orifices of various shapes, including the rectangle, the equilateral triangle, and the square, have been carefully investigated and figured by Magnus.* Phenomena of this kind are of every-day occurrence, and may generally be observed whenever liquid falls from the lip of a moderately elevated vessel.

Admitting the substantial accuracy of Bidone's explanation of the formation and primary expansion of the sheets or excrescences, we have to inquire into the cause of the subsequent contraction. Bidone attributes it to the viscosity of the fluid, which may certainly be put out of the question. In Magnus's view the cause is "cohesion;" but he does not explain what is to be understood under this designation, and it is doubtful whether he had a clear idea upon the subject. The true explanation appears to have been first given by Buff,† who refers the phenomenon distinctly to the capillary force. Under the operation of this force the fluid behaves as if enclosed in an envelope of constant tension, and the recurrent form of the jet is due to vibrations of the fluid column about the circular figure of equilibrium, superposed upon the general progressive motion. Since the phase of vibration depends upon the time elapsed, it is always the same at the same point in space, and thus the motion is *steady* in the hydro-dynamical sense, and the boundary of the jet is a fixed surface.

In so far as the vibrations may be considered to be isochronous, the

* "Hydraulische Untersuchungen." "Pogg. Ann.," xcv, 1855.

† "Pogg. Ann.," Bd. c, 1857.

distance between consecutive corresponding points of the recurrent figure, or, as it may be called, the *wave-length* of the figure, is directly proportional to the velocity of the jet, *i.e.*, to the *square root* of the head of water. This elongation of wave-length with increasing pressure was observed by Bidone and by Magnus, but no definite law was arrived at. As a jet falls under the action of gravity, its velocity increases, and thus an augmentation of wave-length might be expected; but, as will appear later, most of this augmentation is compensated by a change in the frequency of vibration due to the attenuation which is the necessary concomitant of the increased velocity. Consequently but little variation in the magnitudes of successive wave-lengths is to be noticed, even in the case of jets falling vertically with small initial velocity. In the following experiments the jets issued horizontally from orifices in thin plates, usually adapted to a large stoneware bottle, which served as reservoir or cistern. The plates were of tin, soldered to the ends of short brass tubes rather more than an inch in diameter, by the aid of which they could be conveniently fitted to a tubulure in the lower part of the bottle. The pressure at any moment of the outflow could be measured by a water manometer read with a scale of millimetres. Some little uncertainty necessarily attended the determination of the zero point; it was usually taken to be the reading of the scale at which the jet ceased to clear itself from the plate on the running out of the water. At the beginning of an experiment, the orifice was plugged with a small roll of clean paper, and the bottle was filled from an india-rubber tube in connexion with a tap. After a sufficient time had elapsed for the water in the bottle to come sensibly to rest, the plug was withdrawn, and the observations were commenced. The jet is exceedingly sensitive to disturbances in the reservoir, and no arrangement hitherto tried for maintaining the level of the water has been successful. The measurements of wave-length (λ) were made with the aid of a pair of dividers adjusted so as to include one or more wave-lengths; and as nearly as possible at the same moment the manometer was read. The distance between the points of the dividers was afterwards taken from a scale of millimetres. The facility, and in some cases the success, of the operation of observing the wave-length depends very much upon the suitability of the illumination.

The first set of observations here given refers to a somewhat elongated orifice of rectangular form. The pressures and wave-lengths are measured in millimetres. The third column contains numbers proportional to the square roots of the pressures.

Table I.—November 11, 1878.

Pressure.	Wave-length.	√(Pressure).
253	104	91
216	91	84
178	81	76
144	70	69
113*	61*	61*
83	51	52
58	42	43
39	33	36
21	24	26

The agreement of the second and third columns is pretty good on the whole. Small discrepancies at the bottom of the table may be due to the uncertainty attaching to the zero point of pressure, and also to another cause, which will be referred to later. At the higher pressures the observed wave-lengths have a marked tendency to increase more rapidly than the velocity of the jet. This result, which was confirmed by other observations, points to a departure from the law of isochronous vibration. Strict isochronism is only to be expected when vibrations are infinitely small, that is, in the present application when the section of the jet never deviates more than infinitesimally from the circular form. During the vibrations with which Table I is concerned, however, the departures from circularity are very considerable, and there is no reason for supposing that such vibrations will be executed in exactly the same time as vibrations of infinitely small amplitude. Nevertheless, this consideration would not lead to an explanation of the discrepancies in Table I, unless it were the fact that the amplitude of vibration increased with the pressure under which the jet issues.

As a matter of observation the increase of amplitude is very apparent, and was noticed by Magnus. It is also a direct consequence of theory, inasmuch as the *lateral* velocities to which the vibrations are due vary in direct proportion to the longitudinal velocity of the jet. Consequently the amplitude varies approximately as the square root of the pressure, or as the wave-length. The amplitude here spoken of is measured, of course, by the *departure* from circularity, and not by the value of the maximum radius itself.

The law of the square root of the pressure thus applies only to small amplitudes, and unfortunately it is precisely these small amplitudes which it is difficult to experiment upon. Still it is possible to approach theoretical requirements more nearly than in the experiments of Table I.

The next set of measurements (Table II) refer to an aperture in the form of an ellipse of moderate eccentricity. Two wave-lengths were

included in the measurements; in other respects the arrangements were as before.

Table II.—November 12.

Pressure.	Wave-length.	√(Pressure.)
262	40½	40
208	36½	35½
182	34	33½
158	31	31
129	28½	28
107*	25½*	25½*
86	22½	23
69	20	20½
56	18	18½
42	15	16
34	13½	14½
27	12½	13
21	10	11½

In this case the law is fully verified, the discrepancies being decidedly within the limits of experimental error.

On the other hand, the discrepancies may be exaggerated by the use of higher pressures. Table III relates to the same orifice† as Table I. Instead of the stoneware bottle, a tall wooden box was used as reservoir.

Table III.—December 20.

Pressure.	Wave-length.	√(Pressure).
757	200	155
672	184	145
587	171	136
497	152	125
442	141	118
365	123	107
289	106	95½
234	93	86
189	79	77
154	70	70
123	62	62
107	58	58
89*	53*	53*
74	48	48
61	44½	44

The wave-lengths at the high pressures very greatly exceed those calculated from the lower pressures according to the law applicable to small vibrations.

† Its condition may have changed a little in the interval.

It is possible, however, to observe in cases where the amplitude is so small, that the discrepancies are moderate even at higher pressures than those recorded in Table III. The measurements in Table IV are of a jet from an elliptical aperture of small eccentricity. The ratio of axes is about 5 : 6. The wooden box was used. Two wave-lengths were measured.

Table IV.—December 18.

Pressure.	Wave-length.	√(Pressure).
1287	79†	$83\frac{1}{2}$
1195	82	80
1117	$79\frac{1}{2}$	$77\frac{1}{2}$
1023	73	$74\frac{1}{2}$
947	$70\frac{1}{2}$	$71\frac{1}{2}$
852	$66\frac{3}{4}$	68
770	$64\frac{1}{2}$	$64\frac{1}{2}$
695	$61\frac{1}{4}$	61
620	58	58
532	$54\frac{1}{2}$	$53\frac{1}{2}$
451	$48\frac{1}{4}$	$49\frac{1}{4}$
371	$45\frac{1}{2}$	$44\frac{3}{4}$
290*	$39\frac{1}{2}$*	$39\frac{1}{2}$*
248	$36\frac{1}{2}$	$36\frac{1}{2}$
192	$31\frac{1}{2}$	32
158	$28\frac{3}{4}$	$29\frac{1}{4}$
133	$26\frac{1}{2}$	$26\frac{3}{4}$
111	$24\frac{1}{4}$	$24\frac{1}{2}$
94	$21\frac{3}{4}$	$22\frac{1}{4}$
85	21	$21\frac{1}{2}$

The following experiments relate to an orifice in the form of an equilateral triangle, with slightly rounded corners. The side measures about 3 millims. In this case the peculiarities of the contour are repeated *three* times in passing round the circumference. Two wave-lengths were measured.

Table V.—November 16.

Pressure.	Wave-length.	√(Pressure).
215	36	35
166	$31\frac{1}{2}$	31
127	27	27
92*	23*	23*
66	19	$19\frac{1}{2}$
43	$14\frac{1}{2}$	$15\frac{1}{2}$
27	$11\frac{1}{2}$	$12\frac{1}{2}$

† This is, doubtless, an error. At these high pressures the observation is difficult.

Here again we observe the tendency of the wave-length to increase more rapidly than the square root of the pressure.

At higher pressures the difference is naturally still more marked. With the same aperture, and the wooden box as reservoir, the results were:—

Table VI.—December 17.

Pressure.	Wave-length.	√(Pressure).
1072	102	80·4
992	94	77·5
888	89	73·2
827	86	70·7
762	81	67·8
702	77	65·0
619	70	61·1
539	66	57·0
468	59½	53·1
415	54½	50·0
337	47	44·6
292	42	42·0
251	38½	38·9
213	34½	35·8
189	33	33·7
163	31	31·3
140	28¼	29·1
111	24¾	25·9
90	22¾	23·3
70	19¾	20·6
57	17¼	18·5
45	16½	16·5

The wave-lengths down to 34½ are immediate measurements; those below are deduced from measurements of two wave-lengths.

Similar experiments were made with jets from a *square* hole (side = 2 millims.), the peculiarities of which are repeated *four* times in passing round the circumference. Two wave-lengths were measured.

Table VII.—December 14.

Pressure.	Wave-length.	√(Pressure).	Corrected.
447	32	30·2	29·9
377	29½	27·7	27·4
312	27	25·2	24·9
269	24½	23·4	23·1
247	23	22·5	22·1
218	21½	21·1	20·7

Pressure.	Wave-length.	√(Pressure).	Corrected.
192	20	19·8	19·3
167*	18½*	18·5*	18·0
136	16½	16·6	16·1
107	14	14·8	14·2
87	13	13·3	12·7
65	10¾	11·5	10·8
47	8½	9·8	8·9

The third column contains numbers proportional to the square roots of the pressures. In the fourth column a correction is introduced, the significance of which will be explained later.

The value of λ, other things being the same, depends upon the nature of the fluid. Thus methylated alcohol gave a wave-length about twice as great as tap water. This is a consequence of the smaller capillarity.

If a water jet be touched by a fragment of wood moistened with oil, the waves in front of the place of contact are considerably drawn out; but no sensible effect appears to be propagated up the stream.

If a jet of mercury discharging into dilute sulphuric acid be polarized by an electric current, the change in the capillary constant discovered by Lipmann shows itself by alterations in the length of the wave.

When the wave-length is considerable in comparison with the diameter of the jet, the vibrations about the circular form take place practically in two dimensions, and are easily calculated mathematically. The more general case, in which there is no limitation upon the magnitude of the diameter, involves the use of Bessel's functions. The investigation will be found in Appendix I. For the present we will confine ourselves to a statement of the results for vibrations in two dimensions.

Let us suppose that the polar equation of the section is

$$r = a_0 + a_n \cos n\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

so that the curve is an undulating one, repeating itself n times over the circumference. The mean radius is a_0; and, since the deviation from the circular form is small, a_n is a small quantity in comparison with a_0. The vibration is expressed by the variation of a_n as a harmonic function of the time. Thus if $a_n \propto \cos(pt - \epsilon)$, it may be proved that

$$p = \pi^{\frac{1}{2}} T^{\frac{1}{2}} \rho^{-\frac{1}{2}} A^{-\frac{3}{2}} \sqrt{(n^3 - n)} \quad . \quad . \quad . \quad . \quad . \quad (2).$$

In this equation T is the superficial tension, ρ the density, A the area of the section (equal to πa_0^2), and the frequency of vibration is $p \div 2\pi$.

For a jet of given fluid and of given area, the frequency of vibration varies as $\sqrt{(n^3 - n)}$ or $\sqrt{(n-1)n(n+1)}$. The case of $n = 1$ corre-

sponds to a displacement of the jet as a whole, without alteration in the *form* of the boundary. Accordingly there is no potential energy, and the frequency of vibration is zero. For $n=2$ the boundary is elliptical, for $n=3$ triangular with rounded corners, and so on. With most forms of orifice the jet is subject to more than one kind of vibration at the same time. Thus with a square orifice vibrations would occur corresponding to $n=4$, $n=8$, $n=12$, &c. However, the higher modes of vibrations are quite subordinate, and may usually be neglected. The values of $\sqrt{(n^3-n)}$ for various values of n are shown below.

$$
\begin{aligned}
n=\ 2,&\quad p=\sqrt{6} &&=\sqrt{6}\times\ 1\\
n=\ 3,&\quad p=\sqrt{6}\times\sqrt{\ 4} &&=\sqrt{6}\times\ 2\\
n=\ 4,&\quad p=\sqrt{6}\times\sqrt{10} &&=\sqrt{6}\times\ 3{\cdot}16\\
n=\ 5,&\quad p=\sqrt{6}\times\sqrt{20} &&=\sqrt{6}\times\ 4{\cdot}47\\
n=\ 6,&\quad p=\sqrt{6}\times\sqrt{35} &&=\sqrt{6}\times\ 5{\cdot}92\\
n=\ 7,&\quad p=\sqrt{6}\times\sqrt{56} &&=\sqrt{6}\times\ 7{\cdot}48\\
n=\ 8,&\quad p=\sqrt{6}\times\sqrt{84} &&=\sqrt{6}\times\ 9{\cdot}17\\
n=\ 9,&\quad p=\sqrt{6}\times\sqrt{120} &&=\sqrt{6}\times 10{\cdot}95\\
n=12,&\quad p=\sqrt{6}\times\sqrt{286} &&=\sqrt{6}\times 16{\cdot}95
\end{aligned}
$$

It appears that the frequency for $n=3$ is just double that for $n=2$; so that the wave-length for a triangular jet should be the half of that of an elliptical jet of equal area, the other circumstances being the same.

For a given fluid and mode of vibration (n), the frequency varies as $A^{-\frac{1}{2}}$, the thicker jet having the longer time of vibration. If v be the velocity of the jet, $\lambda=2\pi v p^{-1}$. If the jet convey a given volume of fluid, $v \propto A^{-1}$, and thus $\lambda \propto A^{-\frac{1}{2}}$. Accordingly in the case of a jet falling vertically, the increase of λ due to velocity is in great measure compensated by the decrease due to diminishing area of section.

The law of variation of p for a given mode of vibration with the nature of the fluid, and the area of the section, may be found by considerations of *dimensions*. T is a force divided by a line, so that its dimensions are 1 in mass, 0 in length, and -2 in time. The volume density ρ is of 1 dimension in mass, -3 in length, and 0 in time. A is of course of 2 dimensions in length, and 0 in mass and time. Thus the only combination of T, ρ, A, capable of representing a frequency, is $T^{\frac{1}{2}}\rho^{-\frac{1}{2}}A^{-\frac{1}{2}}$.

The above reasoning proceeds upon the assumption of the applicability of the law of isochronism. In the case of large vibrations, for which the law would not be true, we may still obtain a good deal of information by the method of dimensions. The *shape* of the orifice being given, let us inquire into the nature of the dependence of λ upon T, ρ, A, and P, the pressure under which the jet escapes. The

dimensions of P, a force divided by an area, are 1 in mass, -1 in length, and -2 in time. Assume

$$\lambda \propto T^x \rho^y A^z P^u;$$

then by the method of dimensions we have the following relations among the exponents—

$$x+y+u=0, \qquad -3y+2z-u=0, \qquad -2x-2u=0,$$

whence $u=-x, \quad y=0, \quad z=\frac{1}{2}(1-x)$.

Thus $$\lambda \propto T^x A^{\frac{1}{2}-\frac{1}{2}x} P^{-x} \propto A^{\frac{1}{2}} \left(\frac{T}{PA^{\frac{1}{2}}}\right)^x.$$

The exponent x is undetermined; and since any number of terms with different values of x may occur together, all that we can infer is that λ is of the form

$$\lambda = A^{\frac{1}{2}} f\left(\frac{T}{PA^{\frac{1}{2}}}\right),$$

where f is an arbitrary function, or if we prefer it

$$\lambda = T^{-1} P^1 A^{\frac{1}{2}} F\left(\frac{PA^{\frac{1}{2}}}{T}\right),$$

where F is equally arbitrary. Thus for a given liquid and shape of orifice, there is complete dynamical similarity if the pressure be taken inversely proportional to the linear dimension, and this whether the deviation from the circular form be great or small.

In the case of water Quincke found T=81 on the C.G.S. system of units. On the same system $\rho=1$; and thus we get for the frequency of the gravest vibration ($n=2$),

$$\frac{p}{2\pi} = 3{\cdot}51\, a^{-\frac{3}{2}} = 8{\cdot}28\, A^{-\frac{3}{4}} \quad . \quad . \quad . \quad . \quad (3).$$

For a sectional area of one square centimetre, there are thus 8·28 vibrations per second. To obtain the pitch of middle C ($c'=256$) we should require a diameter

$$2a = \left(\frac{3{\cdot}51}{2{\cdot}56}\right)^{\frac{2}{3}} = {\cdot}115,$$

or rather more than a millimetre.

For the general value of n, we have

$$\frac{p}{2\pi} = 1{\cdot}43\, a^{-\frac{3}{2}} \sqrt{(n^3-n)} = 3{\cdot}38\, A^{-\frac{3}{4}} \sqrt{(n^3-n)} \quad . \quad . \quad (4).$$

If h be the head of water to which the velocity of the jet is due,

$$\lambda = \frac{\sqrt{(2gh)} \,.\, A^{\frac{1}{2}}}{3{\cdot}38 \sqrt{(n^3-n)}} \quad . \quad . \quad . \quad . \quad . \quad (5).$$

In applying this formula it must be remembered that A is the area of the section of the jet, and not the area of the aperture. We might indeed deduce the value of A from the area of the aperture by introduction of a coefficient of contraction (about ·62); but the area of the aperture itself is not very easily measured. It is much better to calculate A from an observation of the quantity of fluid (V), discharged under a measured head (h'), comparable in magnitude with that prevailing when λ is measured. Thus $A=V(2gh')^{-\frac{1}{2}}$. In the following calculations the C.G.S. system of units is employed.

In the case of the elliptical aperture of Table II, the value of A was found in this way to be ·0695. Hence at a head of 10·7 the wave-length should be

$$\lambda=\frac{\sqrt{(2g\times 10{\cdot}7)}\times(\cdot 0695)^{\frac{3}{2}}}{3{\cdot}38\sqrt{6}}=2{\cdot}37,$$

the value of g being taken at 981. The corresponding observed value of λ is 2·55.

Again, in the case of the experiments recorded in Table IV, it was found that A=·0660. Hence for h=29·0 the value of the wave-length should be given by

$$\lambda=\frac{(2g\times 29{\cdot}0)\times(\cdot 0660)^{\frac{3}{2}}}{3{\cdot}38\times\sqrt{6}}=3{\cdot}76.$$

The corresponding observed value is 3·95.

We will next take the triangular orifice of Table V. The value of A was found to be ·154. Hence for a head of 9·2 the value of λ, calculated *à priori*, is

$$\lambda=\frac{\sqrt{(2g\times 9{\cdot}2)}\times(\cdot 154)^{\frac{3}{2}}}{3{\cdot}38\times\sqrt{24}}=1{\cdot}99,$$

as compared with 2·3 found by direct observation.

For the square orifice of Table VII, we have A=·153. Hence, if h=16·7,

$$\lambda=\frac{\sqrt{(2g\times 16{\cdot}7)}\times(\cdot 153)^{\frac{3}{2}}}{3{\cdot}38\times\sqrt{60}}=1{\cdot}70,$$

as compared with 1·85 by observation.

It will be remarked that in every case the observed value of λ somewhat exceeds the calculated value. The discrepancies are to be attributed, not so much, I imagine, to errors of observation as to excessive amplitude of vibration, involving a departure from the frequency proper to infinitely small amplitudes. The closest agreement is in the case of Table IV, where the amplitude of vibration was smallest. It is also possible that the capillary tension actually

operative in these experiments was somewhat less than that determined by Quincke for distilled water.

When the pressures are small, the wave-lengths are no longer considerable in comparison with the diameter of the jet, and the vibrations cannot be supposed to take place sensibly in two dimensions. The frequency of vibration then becomes itself a function of the wave-length. This question is investigated mathematically in Appendix I. For the case of $n=4$, it is proved that approximately

$$p^2=\frac{60\mathrm{T}}{\rho a^3}\left(1+\frac{11\pi^2a^2}{30\lambda^2}\right).$$

Hence for the aperture of Table VII,

$$\lambda \propto \sqrt{h}\,(1-\cdot088\,\lambda^{-2}),$$

λ being expressed in centimetres. The numbers in the fourth column of the table are calculated according to this formula.

On the other hand at high pressures the frequency becomes a function of the pressure. Since frequency is always an *even* function of amplitude, and in the present application, the square of the amplitude varies as h, the wave-length is given approximately by an expression of the form $\sqrt{h}\ (\mathrm{M}+\mathrm{N}h)$, where M and N are constants. It appears from experiment, and might, I think, have been expected, that frequency *diminishes* as amplitude increases, so that N is *positive*.

When the aperture has the form of an exact circle, and when the flow of fluid in its neighbourhood is unimpeded by obstacles, there is a perfect balance of lateral motions and pressures, and consequently nothing to render the jet in its future course unsymmetrical. Even in this case, however, the phenomena are profoundly modified by the operation of the capillary force. Far from retaining the cylindrical form unimpaired, the jet rapidly resolves itself in a more or less regular manner into detached masses. It has, in fact, been shown by Plateau,* both from theory and experiment, that in consequence of surface-tension the cylinder is an unstable form of equilibrium, when its length exceeds its circumference.

The circumstances attending the resolution of a cylindrical jet into drops have been admirably examined and described by Savart,† and for the most part explained with great sagacity by Plateau. There are, however, a few points which appear not to have been adequately treated hitherto; and in order to explain myself more effectually

* "Statique Expérimentale et Théorique des Liquides soumis aux seules Forces Moléculaires." Paris, 1873.

† "Mémoire sur la Constitution des Veines Liquides lancées par des Orifices Circulaires en mince paroi." Ann. d. Chim., t. liii, 1833.

I propose to pass in review the leading features of Plateau's theory, imparting, where I am able, additional precision.

Let us conceive, then, an infinitely long circular cylinder of liquid, at rest,* and inquire under what circumstances it is stable, or unstable, for small displacements, symmetrical about the axis of figure.

Whatever the deformation of the originally straight boundary of the axial section may be, it can be resolved by Fourier's theorem into deformations of the harmonic type. These component deformations are in general infinite in number, of every wave-length, and of arbitrary phase; but in the first stages of the motion, with which alone we are at present concerned, each produces its effect independently of every other, and may be considered by itself. Suppose, therefore, that the equation of the boundary is

$$r = a + \alpha \cos kz, \quad . \quad . \quad . \quad . \quad . \quad (6),$$

where α is a small quantity, the axis of z being that of symmetry. The wave-length of the disturbance may be called λ, and is connected with k by the equation $k = 2\pi\lambda^{-1}$. The capillary tension endeavours to contract the surface of the fluid; so that the stability, or instability, of the cylindrical form of equilibrium depends upon whether the surface (enclosing a given volume) be greater or less respectively after the displacement than before. It has been proved by Plateau (see also Appendix I) that the surface is greater than before displacement if $ka > 1$, that is, if $\lambda < 2\pi a$; but less, if $ka < 1$, or $\lambda > 2\pi a$. Accordingly, the equilibrium is stable, if λ be less than the circumference; but unstable, if λ be greater than the circumference of the cylinder. Disturbances of the former kind, like those considered in the earlier part of this paper, lead to *vibrations* of harmonic type, whose amplitudes always remain small; but disturbances, whose wave-length exceeds the circumference, result in a greater and greater departure from the cylindrical figure. The analytical expression for the motion in the latter case involves exponential terms, one of which (except in case of a particular relation between the initial displacements and velocities) increases rapidly, being equally multiplied in equal times. The coefficient (q) of the time in the exponential term (e^{qt}) may be considered to measure the degree of dynamical instability; its reciprocal q^{-1} is the time in which the disturbance is multiplied in the ratio $1 : e$.

The degree of instability, as measured by q, is not to be determined from statical considerations only; otherwise there would be no limit to the increasing efficiency of the longer wave-lengths. The joint operation of superficial tension and *inertia* in fixing the wave-

* A motion common to every part of the fluid is necessarily without influence upon the stability, and may therefore be left out of account for convenience of conception and expression.

length of maximum instability was, I believe, first considered in a communication to the Mathematical Society,* on the "Instability of Jets." It appears that the value of q may be expressed in the form

$$q=\sqrt{\left(\frac{T}{\rho a^3}\right)}\ .\ F(ka)\ .\ .\ .\ .\ .\ (7),$$

where, as before, T is the superficial tension, ρ the density, and F is given by the following table:—

k^2a^2.	F(ka).	k^2a^2.	F(ka).
·05	·1536	·4	·3382
·1	·2108	·5	·3432
·2	·2794	·6	·3344
·3	·3182	·8	·2701
		·9	·2015

The greatest value of F thus corresponds, not to a zero value of k^2a^2, but approximately to $k^2a^2=·4858$, or to $\lambda=4·508\times 2a$. Hence the maximum instability occurs when the wave-length of disturbance is about half as great again as that at which instability first commences.

Taking for water, in C.G.S. units, T=81, $\rho=1$, we get for the case of maximum instability,

$$q^{-1}=\frac{a^{\frac{3}{2}}}{81\times ·343}=·115\,d^{\frac{3}{2}}\ .\ .\ .\ .\ (8),$$

if d be the diameter of the cylinder. Thus, if $d=1$, $q^{-1}=·115$; or for a diameter of one centimetre the disturbance is multiplied 2·7 times in about one-ninth of a second. If the disturbance be multiplied 1000 fold in time t, $qt=3\ log_e\ 10=6·9$, so that $t=·79d^{\frac{3}{2}}$. For example, if the diameter be one millimetre, the disturbance is multiplied 1000 fold in about one-fortieth of a second. In view of these estimates the rapid disintegration of a fine jet of water will not cause surprise.

The relative importance of two harmonic disturbances depends upon their initial magnitudes, and upon the rate at which they grow. When the initial values are very small, the latter consideration is much the more important; for, if the disturbances be represented by $\alpha_1 e^{q_1 t}$, $\alpha_2 e^{q_2 t}$, in which q_1 exceeds q_2, their ratio is $\frac{\alpha_2}{\alpha_1}e^{-(q_1-q_2)t}$; and this ratio decreases without limit with the time, whatever be the initial (finite) ratio $\alpha_2 : \alpha_1$. If the initial disturbances are small enough, that one is ultimately preponderant, for which the measure of instability is

* "Math. Soc. Proc.," November, 1878. See also Appendix I.

greatest. The smaller the causes by which the original equilibrium is upset, the more will the cylindrical mass tend to divide itself regularly into portions whose length is equal to 4·5 times the diameter. But a disturbance of less favourable wave-length may gain the preponderance in case its magnitude be sufficient to produce disintegration in a less time than that required by the other disturbances present.

The application of these results to actual jets presents no great difficulty. The disturbances, by which equilibrium is upset, are impressed upon the fluid as it leaves the aperture, and the continuous portion of the jet represents the distance travelled during the time necessary to produce disintegration. Thus the length of the continuous portion necessarily depends upon the character of the disturbances in respect of amplitude and wave-length. It may be increased considerably, as Savart showed, by a suitable isolation of the reservoir from tremors, whether due to external sources or to the impact of the jet itself in the vessel placed to receive it. Nevertheless it does not appear to be possible to carry the prolongation very far. Whether the residuary disturbances are of external origin, or are due to friction, or to some peculiarity of the fluid motion within the reservoir, has not been satisfactorily determined. On this point Plateau's explanations are not very clear, and he sometimes expresses himself as if the time of disintegration depended only upon the capillary tension, without reference to initial disturbances at all.

Two laws were formulated by Savart with respect to the length of the continuous portion of a jet, and have been to a certain extent explained by Plateau. For a given fluid and a given orifice the length is approximately proportional to the square root of the head. This follows at once from theory, if it can be assumed that the disturbances remain always of the same character, so that the *time* of disintegration is constant.* When the head is given, Savart found the length to be proportional to the diameter of the orifice. From (8) it appears that the time in which a disturbance is multiplied in a given ratio varies, not as d, but as $d^{\frac{3}{2}}$. Again, when the fluid is changed, the time varies as $\rho^{\frac{1}{2}}\,T^{-\frac{1}{2}}$. But it may be doubted, I think, whether the length of the continuous portion obeys any very simple laws, even when external disturbances are avoided as far as possible.

When the circumstances of the experiment are such that the reservoir is influenced by the shocks due to the impact of the jet, the disintegration usually establishes itself with complete regularity, and is attended by a musical note (Savart). The impact of the regular series of drops which is at any moment striking the sink (or vessel receiving the water), determines the rupture into similar drops of the portion of the jet at the same moment passing the orifice. The pitch

* For the sake of simplicity, I neglect the action of gravity upon the jet when formed. The question has been further discussed by Plateau.

of the note, though not absolutely definite, cannot differ much from that which corresponds to the division of the jet into wave-lengths of maximum instability; and, in fact, Savart found that the frequency was directly as the square root of the head, inversely as the diameter of the orifice, and independent of the nature of the fluid—laws which follow immediately from Plateau's theory.

From the pitch of the note due to a jet of given diameter, and issuing under a given head, the wave-length of the nascent divisions can be at once deduced. Reasoning from some observations of Savart, Plateau finds in this way 4·38 as the ratio of the length of a division to the diameter of the jet. The diameter of the orifice was 3 millims., from which that of the jet is deduced by the introduction of the coefficient ·8. Now that the length of a division has been estimated *à priori*, it is perhaps preferable to reverse Plateau's calculation, and to exhibit the frequency of vibration in terms of the other data of the problem. Thus

$$\text{frequency} = \frac{\sqrt{(2gh)}}{4{\cdot}508\,d} \quad . \quad . \quad . \quad . \quad . \quad (9).$$

But the most certain method of obtaining complete regularity of resolution is to bring the reservoir under the influence of an external vibrator, whose pitch is approximately the same as that proper to the jet. Magnus* employed a Neef's hammer, attached to the wooden frame which supported the reservoir. Perhaps an electrically maintained tuning-fork is still better. Magnus showed that the most important part of the effect is due to the forced vibration of that side of the vessel which contains the orifice, and that but little of it is propagated through the air. With respect to the limits of pitch, Savart found that the note might be a fifth above, and more than an octave below, that proper to the jet. According to theory, there would be no well-defined lower limit; on the other side, the external vibration cannot be efficient if it tends to produce divisions whose length is less than the circumference of the jet. This would give for the interval defining the upper limit $\pi : 4{\cdot}508$, which is very nearly a fifth. In the case of Plateau's numbers ($\pi : 4{\cdot}38$) the discrepancy is a little greater.

The detached masses into which a jet is resolved do not at once assume and retain a spherical form, but execute a series of vibrations, being alternately compressed and elongated in the direction of the axis of symmetry. When the resolution is effected in a perfectly periodic manner, each drop is in the same phase of its vibration as it passes through a given point of space; and thence arises the remarkable appearance of alternate swellings and contractions described by Savart. The interval from one swelling to the next is the space described by

* "Pogg. Ann.," bd. cvi, 1859.

the drop during one complete vibration, and is therefore (as Plateau shows) proportional *cæteris paribus* to the square root of the head.

The time of vibration is of course itself a function of the nature of the fluid and of the size of the drop. By the method of dimensions alone it may be seen that the time of infinitely small vibrations varies directly as the square root of the mass of the sphere and inversely as the square root of the capillary tension; and in Appendix II it is proved that its expression is

$$\tau = \sqrt{\left(\frac{3\pi\rho V}{8T}\right)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

V being the volume of the vibrating mass.

In an experiment arranged to determine the time of vibration, a stream of 19·7 cub. centims. per second was broken up under the action of a fork making 128 vibrations per second. Neglecting the mass of the small spherules (of which more will be said presently), we get for the mass of each sphere 19·7÷128, or ·154 grm.; and thence by (10), taking as before T=81,

$$\tau = \cdot 0473 \text{ second.}$$

The distance between the first and second swellings was by measurement 16·5 centims. The level of the contraction midway between the two swellings was 36·8 centims. below the surface of the liquid in the reservoir, corresponding to a velocity of 175 centims. per second. These data give for the time of vibration,

$$\tau = 16{\cdot}5 \div 36{\cdot}8 = \cdot 0612 \text{ second.}$$

The discrepancy between the two values of τ, which is greater than I had expected, is doubtless due in part to excessive amplitude, rendering the vibration slower than that calculated for infinitely small amplitudes.

A rough estimate of the degree of flattening to be expected at the first swelling may be arrived at by calculating the eccentricity of the *oblatum*, which has the same volume and *surface* as those appertaining to the portion of fluid in question when forming part of the undisturbed cylinder. In the case of the most natural mode of resolution, the volume of a drop is $9\pi a^3$, and its surface is $18\pi a^2$. The eccentricity of the *oblatum* which has this volume and this surface is ·944, corresponding to a ratio of principal axes equal to about 1 : 3.

In consequence of the rapidity of the motion some optical device is necessary to render apparent the phenomena attending the disintegration of a jet. Magnus employed a rotating mirror, and also a rotating disk from which a fine slit was cut out. The readiest method of obtaining instantaneous illumination is the electric spark, but with this Magnus was not successful. "The rounded masses of which the swellings consist reflect the light emanating from a point in such a

manner that the eye sees only the single point of each, which is principally illuminated. Hence, when the stream is illuminated by the electric spark, the swellings appear like a string of pearls ; but their form cannot be recognised, because the intensity of the light reflected from the remaining portions of the masses is too small to allow this, on account of the velocity with which the impression is lost."* The electric spark had, however, been used successfully for this purpose some years before by Buff,† who observed the *shadow* of the jet on a white screen. Preferable to an opaque screen in my experience is a piece of ground glass, which allows the shadow to be examined from the further side. I have found also that the jet may be very well observed directly, if the illumination is properly managed. For this purpose it is necessary to place the jet between the source of light and the eye. The best effect is obtained when the light of the spark is somewhat diffused by being passed (for example) through a piece of ground glass.

The spark may be obtained from the secondary of an induction coil, whose terminals are in connexion with the coatings of a Leyden jar. By adjustment of the contact breaker the series of sparks may be made to fit more or less perfectly with the formation of the drops. A still greater improvement may be effected by using an electrically maintained fork, which performs the double office of controlling the resolution of the jet and of interrupting the primary current of the induction coil. In this form the experiment is one of remarkable beauty. The jet, illuminated only in one phase of transformation, appears almost perfectly steady, and may be examined at leisure. The fork that I used had a frequency of 128, and communicated its vibration to the reservoir through the table on which both were placed without any special provision for the purpose. The only weak point in the arrangement was the rather feeble character of the sparks, depending probably upon the use of an induction coil too large for the rate of intermittence. A change in the phase under observation could be effected by pressing slightly upon the reservoir, whereby the vibration communicated was rendered more or less intense.

The jet issued horizontally from an orifice of about half a centimetre in diameter, and almost immediately assumed a rippled outline. The gradually increasing amplitude of the disturbance, the formation of the elongated ligament, and the subsequent transformation of the ligament into a spherule, could be examined with ease. In consequence of the transformation being in a more advanced stage at the forward than at the hinder end, the ligament remains for a moment connected with the mass behind, when

* "Phil. Mag.," xviii, 1859, p. 172.

† "Liebig's Ann.," lxxviii, 1851.

it has freed itself from the mass in front, and thus the resulting spherule acquires a backwards relative velocity, which of necessity leads to a collision. Under ordinary circumstances the spherule rebounds, and may be thus reflected backwards and forwards several times between the adjacent masses. But if the jet be subject to moderate electrical influence, the spherule amalgamates with a larger mass at the first opportunity.* Magnus showed that the stream of spherules may be diverted into another path by the attraction of a powerfully electrified rod, held a little below the place of resolution.

Very interesting modifications of these phenomena are observed when a jet from an orifice in a thin plate† is directed obliquely upwards. In this case drops which break away with different velocities are carried under the action of gravity into different paths; and thus under ordinary circumstances a jet is apparently resolved into a "sheaf," or bundle of jets all lying in one vertical plane. Under the action of a vibrator of suitable periodic time the resolution is regularised, and then each drop, breaking away under like conditions, is projected with the same velocity, and therefore follows the same path. The apparent gathering together of the sheaf into a fine and well-defined stream is an effect of singular beauty.

In certain cases where the tremor to which the jet is subjected is compound, the single path is replaced by two, three, or even more paths, which the drops follow in a regular cycle. The explanation has been given with remarkable insight by Plateau. If for example besides the principal disturbance, which determines the size of the drops, there be another of twice the period, it is clear that the alternate drops break away under different conditions and therefore with different velocities. Complete periodicity is only attained after the passage of a *pair* of drops; and thus the odd series of drops pursues one path, and the even series another. All I propose at present is to bring forward a few facts connected with the influence of electricity, which are not mentioned in my former communication. To it, however, I must refer the reader for further explanations. The literature of the subject is given very fully in Plateau's second volume.

When the jet is projected upwards at a moderate obliquity, the sheaf is (as Savart describes it) confined to a vertical plane. Under these circumstances, there are few or no collisions, as the drops have room to clear one another, and moderate electrical influence is without effect. At a higher obliquity the drops begin to be scattered out of the vertical plane, which is a sign that collisions are taking place. Moderate electrical influence will now reduce the scattering again to

* "Proc. Roy. Soc," March 13, 1879. On the Influence of Electricity on Colliding Water Drops.

† Tyndall has shown that a pinhole gas burner may also be used with advantage.

the vertical plane, by causing the coalescence of drops which come into contact. When the projection is nearly vertical, the whole scattering is due to collisions, and is destroyed by electricity. If the resolution into drops is regularised by vibrations of suitable frequency, the principal drops follow the same path, and unless the projection is nearly vertical, there are no collisions, as explained in my former paper. It sometimes happens that the spherules are projected laterally in a distinct stream, making a considerable angle with the main stream. This is the result of collisions between the spherules and the principal drops. I believe that the former are often reflected backwards and forwards several times, until at last they escape laterally. Occasionally the principal drops themselves collide in a regular manner, and ultimately escape in a double stream. In all cases the behaviour under electrical influence is a criterion of the occurrence of collisions. The principal phenomena are easily observed directly, with the aid of instantaneous illumination.

Appendix I.

The subject of this appendix is the mathematical investigation of the motion of frictionless fluid under the action of capillary force, the configuration of the fluid differing infinitely little from that of equilibrium in the form of an infinite circular cylinder.

Taking the axis of the cylinder as axis of z, and polar co-ordinate r,θ in the perpendicular plane, we may express the form of the surface at any time t by the equation

$$r=a_0+f(\theta,z) \quad . \quad . \quad . \quad . \quad . \quad . \quad (11),$$

in which $f(\theta,z)$ is always a small quantity. By Fouriers' theorem, the arbitrary function f may be expanded in a series of terms of the type $\alpha_n \cos n\theta \cos kz$; and, as we shall see in the course of the investigation, each of these terms may be considered independently of the others. The summation extends to all positive values of k, and to all positive integral values of n, zero included.

During the motion the quantity a_0 does not remain absolutely constant, and must be determined by the condition that the inclosed *volume* is invariable. Now for the surface.

$$r=a_0+\alpha_n \cos n\theta \cos kz \quad . \quad . \quad . \quad . \quad . \quad (12),$$

we find

$$\text{volume}=\tfrac{1}{2}\iint r^2 d\theta dz=\int(\pi a_0^2+\tfrac{1}{2}\pi\alpha_n^2\cos^2 kz)dz=z(\pi a_0^2+\tfrac{1}{4}\pi\alpha_n^2);$$

so that, if a denote the radius of the section of the undisturbed cylinder,

$$\pi a^2=\pi a_0^2+\tfrac{1}{4}\pi\alpha_n^2,$$

whence approximately

$$a_0=a\left(1-\frac{\alpha_n^2}{8a^2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13).$$

For the case $n=0$, (13) is replaced by

$$a_0=a\left(1-\frac{\alpha_0^2}{4a^2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14).$$

We have now to calculate the area of the surface of (22), on which the potential energy of displacement depends. We have

$$\text{Surface}=\iint\left\{1+\left(\frac{dr}{dz}\right)^2+\left(\frac{dr}{rd\theta}\right)^2\right\}^{\frac{1}{2}} r\,d\theta\,dz$$

$$=\iint\left\{1+\frac{1}{2}\left(\frac{dr}{dz}\right)^2+\frac{1}{2r^2}\left(\frac{dr}{d\theta}\right)^2\right\} r\,d\theta\,dz$$

$$=\iint\{1+\tfrac{1}{2}k^2\alpha_n^2\cos^2n\theta\sin^2kz+\tfrac{1}{2}n^2\alpha_n^2a^{-2}\sin^2n\theta\cos^2kz\}r\,d\theta\,dz$$

$$=z\{2\pi a_0+\tfrac{1}{4}\pi k^2\alpha_n^2a+\tfrac{1}{4}\pi n^2\alpha_n^2a^{-1}\};$$

so that, if a denote the surface corresponding on the average to the unit of length,

$$\sigma=2\pi a+\tfrac{1}{4}\pi a^{-1}(k^2a^2+n^2-1)\alpha_n^2 \quad . \quad . \quad . \quad (15),$$

the value of a_0 being substituted from (13).

The potential energy P, estimated per unit length, is therefore expressed by

$$\mathrm{P}=\tfrac{1}{4}\pi a^{-1}\mathrm{T}(k^2a^2+n^2-1)\alpha_n^2 \quad . \quad . \quad . \quad . \quad (16),$$

T being the superficial tension.

For the case $n=0$, (16) is replaced by

$$\mathrm{P}=\tfrac{1}{2}\pi a^{-1}\mathrm{T}(k^2a^2-1)\alpha_0^2 \quad . \quad . \quad . \quad . \quad . \quad (17).$$

From (16) it appears that, when n is unity or any greater integer, the value of P is positive, showing that, for all displacements of these kinds, the original equilibrium is stable. For the case of displacements symmetrical about the axis, we see from (17) that the equilibrium is stable or unstable according as ka is greater or less than unity, *i.e.*, according as the wave-length $(2\pi k^{-1})$ is less or greater than the circumference of the cylinder.

If the expression for r in (12) involve a number of terms with various values of n and k, the corresponding expression for P is found by simple addition of the expressions relating to the component terms, and contains only the squares (and not the products) of the quantities α.

The velocity potential (ϕ) of the motion of the fluid satisfies the equation

$$\frac{d^2\phi}{dr^2}+\frac{1}{r}\frac{d\phi}{dr}+\frac{1}{r^2}\frac{d^2\phi}{d\theta^2}+\frac{d^2\phi}{dz^2}=0;$$

or, if in order to correspond with (12) we assume that the variable part is proportional to $\cos n\theta \cos kz$,

$$\frac{d^2\phi}{dr^2}+\frac{1}{r}\frac{d\phi}{dr}-\left(\frac{n^2}{r^2}+k^2\right)\phi=0 \quad . \quad . \quad . \quad . \quad (18).$$

The solution of (18) under the condition that there is no introduction or abstraction of fluid along the axis of symmetry is—

$$\phi=\beta_n \mathrm{J}_n(ikr)\cos n\theta \cos kz \quad . \quad . \quad . \quad . \quad (19),$$

in which $i=\sqrt{(-1)}$, and J_n is the symbol of the Bessel's function of the nth order, so that

$$\mathrm{J}_n(ikr)=\frac{(kr)^n}{2^n\Gamma(n+1)}\left\{1+\frac{k^2r^2}{2\,.\,2n+2}+\frac{k^4r^4}{2\,.\,4\,.\,2n+2\,.\,2n+4}\right.$$
$$\left.+\frac{k^6r^6}{2\,.\,4\,.\,6\,.\,2n+2\,.\,2n+4\,.\,2n+6}+\;.\;.\right\}\;.\;.\quad(20).$$

The constant β_n is to be found from the condition that the radial velocity when $r=a$ coincides with that implied in (12). Thus

$$ik\,\beta_n \mathrm{J}_n'(ika)=\frac{d\alpha_n}{dt} \quad . \quad . \quad . \quad . \quad . \quad (21).$$

The kinetic energy of the motion is, by Green's theorem,

$$\tfrac{1}{2}\rho\iint\left[\phi\frac{d\phi}{dr}\right]_{r=a} a\,d\theta\,dz=\tfrac{1}{4}\pi\rho z\,.\,ika\,.\,\mathrm{J}_n(ika)\,\mathrm{J}_n'(ika)\,.\,\beta_n{}^2;$$

so that, by (21), if K denote the kinetic energy per unit length,

$$\mathrm{K}=\tfrac{1}{4}\pi\rho a^2\frac{\mathrm{J}_n(ika)}{ika\,.\,\mathrm{J}_n'(ika)}\left(\frac{d\alpha_n}{dt}\right)^2 \quad . \quad . \quad . \quad (22).$$

When $n=0$, we must take, instead of (22),

$$\mathrm{K}=\tfrac{1}{2}\pi\rho a^2\frac{\mathrm{J}_0(ika)}{ika\,.\,\mathrm{J}_0'(ika)}\left(\frac{d\alpha_0}{dt}\right)^2 \quad . \quad . \quad . \quad (23).$$

The most general value of K is to be found by simple summation, with respect to n and k, from the particular values expressed in (22) and (23). Since the expressions for P and K involve only the squares, and not the products, of the quantities α, $\frac{d\alpha}{dt}$, it follows that the

motions represented by (12) take place in perfect independence of one another.

For the free motion we get by Lagrange's method from (16), (22),

$$\frac{d^2a_n}{dt^2}+\frac{T}{\rho a^3}\frac{ika\,.\,J_n'(ika)}{J_n(ika)}(n^2+k^2a^2-1)\alpha_n=0 \quad . \quad . \quad (24),$$

which applies without change to the case $n=0$. Thus, if $\alpha_n \propto \cos(pt-\epsilon)$,

$$p^2=\frac{T}{\rho a^3}\frac{ika\,J_n'(ika)}{J_n(ika)}(n^2+k^2a^2-1) \quad . \quad . \quad . \quad (25),$$

giving the frequency of vibration in the cases of stability. If $n=0$, and $ka<1$, the solution changes its form. If we suppose that $\alpha_n \propto e^{\pm qt}$,

$$q^2=\frac{T}{\rho a^3}\frac{ika\,J'_0(ika)}{J_0(ika)}(1-k^2a^2) \quad . \quad . \quad . \quad (26).$$

From this the table in the text was calculated.

When n is greater than unity, the values of p^2 in (25) are usually in practical cases nearly the same as if ka were zero, or the motion took place in two dimensions. We may therefore advantageously introduce into (25) the supposition that ka is small. In this way we get

$$p^2=n(n^2-1+k^2a^2)\frac{T}{\rho a^3}\left[1+\frac{k^2a^2}{n\,.\,2n+2}+-\right] \quad . \quad (27),$$

or, if ka be neglected altogether,

$$p^2=(n^3-n)\frac{T}{\rho a^3} \quad . \quad . \quad . \quad . \quad . \quad (28),$$

which agree with the formulæ used in the text. When $n=1$, there is no force of restitution for the case of a displacement in two dimensions.

Combining in the usual way two stationary vibrations, whose phases differ by a quarter of a period, we find as the expression of a progressive wave,

$$r=a_0+\gamma_n\cos n\theta\cos kz\cos pt+\gamma_n\cos n\theta\sin kz\sin pt$$

$$=a_0+\gamma_n\cos n\theta\cos(pt-kz) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29).$$

For the application to a jet the progressive wave must be reduced to steady motion by the superposition of a common velocity (v) equal and opposite to that of the wave's propagation. The solution then becomes

$$r=a_0+\gamma_n\cos n\theta\cos kz \quad . \quad . \quad . \quad . \quad (30),$$

in which γ_n is an absolute constant. The corresponding velocity-potential is

$$\phi = -vz + \frac{p\,\gamma_n \mathrm{J}_n(ikr)}{ik\mathrm{J}_n'(ika)} \sin kz \cos n\theta \quad . \quad . \quad . \quad (31).$$

It is instructive to verify these results by the formulæ applicable to steady motion. The resultant velocity q at any point is approximately equal to $\frac{d\phi}{dz}$; and

$$\frac{d\phi}{dz} = -v + \frac{p\,k\,\gamma_n \mathrm{J}_n(ikr)}{ik\mathrm{J}_n'(ika)} \cos kz \cos n\theta.$$

At the surface we have approximately $r=a$, and

$$\tfrac{1}{2}q^2 = \tfrac{1}{2}v^2 - p\,k\,v\frac{\gamma_n \mathrm{J}_n(ika)}{ik\,\mathrm{J}'(ika)} \cos kz \cos n\theta.$$

Thus by the hydrodynamical equation of pressure, with use of (25), since $v=pk^{-1}$,

$$\text{Pressure} = \text{const.} + \gamma_n a^{-2} \mathrm{T}\,(n^2 - 1 + k^2a^2) \cos kz \cos n\theta \quad . \quad (32).$$

The pressure due to superficial tension is $\mathrm{T}\,(\mathrm{R}_1^{-1} + \mathrm{R}_2^{-1})$, if R_1, R_2, are the radii of curvature in planes parallel and perpendicular to the axis; and from (30)

$$-\mathrm{R}_2^{-1} = \frac{d^2r}{dz^2} = -k^2\gamma_n \cos n\theta \cos kz$$

$$\mathrm{R}_1^{-1} = r^{-1} + \frac{d^2r^{-1}}{d\theta^2} = a^{-1} + \gamma_n a^{-2}(n^2-1)\cos n\theta \cos kz\,;$$

so that

$$\text{Pressure} = \text{const.} + \gamma_n a^{-2}(n^2 - 1 + k^2a^2)\cos n\theta \cos kz.$$

Thus the pressure due to velocity is exactly balanced by the capillary force, and the surface condition of equilibrium is satisfied.

Appendix II.

We will now investigate in the same manner the vibrations of a liquid mass about a *spherical* figure, confining ourselves for brevity to modes of vibrations symmetrical about an axis, which is sufficient for the application in the text. These modes require for their expression only Legendre's functions P_n; the more general problem, involving Laplace's functions, may be treated in the same way, and leads to the same results.

The radius r may be expanded at any time t in the series

$$r = a_0 + a_1\mathrm{P}_1(\mu) + \ . \ . \ . \ + a_n\mathrm{P}_n(\mu) + \ . \ . \quad (33),$$

where a_1 a_2 are small quantities relatively to a_0, and μ (according to the usual notation) represents the cosine of the colatitude (θ).

For the volume included within the surface (33) we have

$$\begin{aligned}
V&=\tfrac{2}{3}\pi\int_{-1}^{+1} r^3 d\mu \\
&=\tfrac{2}{3}\pi\int_{-1}^{+1}[a_0{}^3+3a_0{}^2\{a_1P_1(\mu)+\ldots+a_nP_n(\mu)+\ldots\} \\
&+3a_0\{a_1P_1(\mu)+\ldots+a_nP_n(\mu)+\ldots\}^2+\ldots]d\mu \\
&=\tfrac{2}{3}\pi a_0{}^3\int_{-1}^{+1}[1+3a_0{}^{-2}\Sigma a_n{}^2P_n{}^2(\mu)]d\mu \\
&=\tfrac{4}{3}\pi a_0{}^3[1+3a_0{}^{-2}\Sigma(2n+1)^{-1}a_n{}^2,
\end{aligned}$$

approximately. If a be the radius of the sphere of equilibrium,

$$a^3=a_0{}^3[1+3a^{-2}\Sigma(2n+1)^{-1}a_n{}^2] \quad . \quad . \quad . \quad (34).$$

We have now to calculate the area of the surface S.

$$S=2\pi\int r\sin\theta\sqrt{\left\{r^2+\left(\frac{dr}{d\theta}\right)^2\right\}}\,d\theta=2\pi\int\left\{r^2+\tfrac{1}{2}\left(\frac{dr}{d\theta}\right)^2\right\}\sin\theta d\theta.$$

For the first part

$$\int_{-1}^{+1} r^2 d\mu=2a_0{}^2+2\Sigma(2n+1)^{-1}a_n{}^2.$$

For the second part

$$\tfrac{1}{2}\int\left(\frac{dr}{d\theta}\right)^2\sin\theta\,d\theta=\tfrac{1}{2}\int_{-1}^{+1}(1-\mu^2)\left[\Sigma a_n\frac{dP_n}{d\mu}\right]^2 d\mu.$$

The value of the quantity on the right hand side may be found with the aid of the formula*

$$\int_{-1}^{+1}(1-\mu^2)\frac{dP_m}{d\mu}\frac{dP_n}{d\mu}\,d\mu=n(n+1)\int_{-1}^{+1}P_mP_n d\mu.$$

Thus

$$\begin{aligned}
\tfrac{1}{2}\int_{-1}^{+1}\left(\frac{dr}{d\theta}\right)^2\sin\theta\,d\theta&=\tfrac{1}{2}\int_{-1}^{+1}(1-\mu^2)\Sigma a_n{}^2\left(\frac{dP_n}{d\mu}\right)^2 d\mu \\
&=\tfrac{1}{2}\Sigma n(n+1)a_n{}^2\int_{-1}^{+1}P_n{}^2 d\mu=\Sigma\, n(n+1)(2n+1)^{-1}a_n{}^2.
\end{aligned}$$

Accordingly $S=4\pi a_0{}^2+2\pi\Sigma(2n+1)^{-1}(n^2+n+2)a_n{}^2$;

or, since by (34)

$$a_0{}^2=a^2-2\Sigma(2n+1)^{-1}a_n{}^2,$$

$$S=4\pi a^2+2\pi\Sigma(n-1)(n+2)(2n+1)^{-1}a_n{}^2 \quad . \quad . \quad (35).$$

* Todhunter's "Laplace's Functions," § 62.

If T be the cohesive tension, the potential energy is

$$P=2\pi T\,\Sigma(n-1)(n+2)(2n+1)^{-1}a_n^2 \quad . \quad . \quad (36).$$

We have now to calculate the kinetic energy of the motion. The velocity-potential ϕ may be expanded in the series

$$\phi=\beta_0+\beta_1 r\,P_1(\mu)+ \quad . \quad . \quad +\beta_n r^n P_n(\mu)+ \quad . \quad (37);$$

and thus for K we get

$$K=\tfrac{1}{2}\rho\iint\phi\frac{d\phi}{dr}dS=\tfrac{1}{2}\rho\,.\,2\pi a^2\int_{-1}^{+1}\phi\frac{d\phi}{dr}d\mu=\tfrac{1}{2}\rho\,.\,4\pi a^2\,.\,\Sigma\,(2n+1)^{-1}n\,a^{2n-1}\beta_n^2.$$

But by comparison of the value of $\frac{d\phi}{dr}$ from (37) with (33), we find

$$n\,a^{n-1}\beta_n=\frac{da_n}{dt}:$$

and thus

$$K=2\pi\rho a^3\Sigma(2n+1)^{-1}n^{-1}\left(\frac{da_n}{dt}\right)^2 \quad . \quad . \quad . \quad (38).$$

Since the products of the quantities a_n and $\frac{da_n}{dt}$ do not occur in the expressions for P and K, the motions represented by the various terms occur independently of one another. The equation for a_n is by Lagrange's method

$$\frac{d^2a_n}{dt^2}+n(n-1)(n+2)\frac{T}{\rho a^3}a_n=0 \quad . \quad . \quad (39);$$

so that, if $a_n \propto \cos(pt+\epsilon)$,

$$p^2=n(n-1)(n+2)\frac{T}{\rho a^3} \quad . \quad . \quad . \quad . \quad . \quad (40).$$

The periodic time τ given in the text (equation (10)) follows from (40) by putting $\tau=2\pi p^{-1}$, $n=2$, $V=\frac{4}{3}\pi a^3$.

To find the radius of the sphere of water which vibrates seconds, put $p=2\pi$, $T=81$, $\rho=1$, $n=2$. Thus $a=2{\cdot}54$ centims., or one inch almost exactly.

The Society adjourned over Ascension Day to Thursday, May 29.

May 29, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Conduction of Heat in Ellipsoids of Revolution." By C. NIVEN, M.A., Professor of Mathematics in Queen's College, Cork. Communicated by J. W. L. GLAISHER, F.R.S. Received May 7, 1879.

(Abstract.)

The object of the present paper is to investigate the expressions which present themselves in the mathematical treatment of the problem of the conduction of heat in an ellipsoid of revolution. The results obtained constitute a generalisation of the corresponding solution for the sphere, and are found, in the first instance, for an ellipsoid whose major axis is the axis of revolution, but a slight alteration will render them applicable also to a planetary ellipsoid. We may effect the transformation to ellipsoidal co-ordinates of the general equation of conduction as follows: the semi-axes of the ellipsoid and hyperboloid through any point confocal to the surface being denoted by $c\cosh\alpha$, $c\sinh\alpha$, and $c\cos\beta$, $c\sin\beta$, the co-ordinates of the point may be expressed by $c\cos\phi\sinh\alpha\sin\beta$, $c\sin\phi\sinh\alpha\sin\beta$, $c\cosh\alpha\cos\beta$, and the general equation of conduction becomes

$$\frac{d^2V}{d\alpha^2}+\frac{d^2V}{d\beta^2}+\coth\alpha\frac{dV}{d\alpha}+\cot\beta\frac{dV}{d\beta}+\left(\frac{1}{\sin^2\beta}+\frac{1}{\sinh^2\alpha}\right)\frac{d^2V}{d\phi^2}$$
$$=\frac{c^2}{k}(\cosh^2\alpha-\cos^2\beta)\frac{dV}{dt},$$

V being the temperature at any point.

We may satisfy this equation by

$$V=(\cos m\phi \text{ or } \sin m\phi)e^{-k^2t}\theta^z_m(\beta)\Omega^z_m(\alpha),$$

where

$$\frac{d^2\theta}{d\beta^2}+\cot\beta\frac{d\theta}{d\beta}-\frac{m^2\theta}{\sin^2\beta}=\lambda^2c^2\cos^2\beta\,\theta-z\theta,$$

$$\frac{d^2\Omega}{d\alpha^2}+\coth\alpha\frac{d\Omega}{d\alpha}-\frac{m^2\Omega}{\sinh^2\alpha}=-\lambda^2c^2\cosh^2\alpha\,\Omega-z\Omega,$$

z being a constant to be determined, the same in both; and we have to determine the appropriate solutions of these equations.

Writing ν for $\cos\beta$, and denoting by $P_m^n(\nu)$ the tesseral harmonic $\frac{(n-m)!}{(2n)!}(\nu^2-1)^{\frac{m}{2}}\frac{d^{m+n}(r^2-1)^n}{d\nu^{m+n}}$, it is proved that the equation in θ is satisfied by expression of the form

$$\theta_m^z = a_0 P_m^m - a_1 P_m^{m+2} + a_2 P_m^{m+4} - \ldots$$

or

$$\theta_m^z = b_0 P_m^{m+1} - b_1 P_m^{m+3} + b_2 P_m^{m+5},$$

and the coefficients (a) and (b) are related as follows—

Putting

$$\lambda^2c^2=\epsilon, \frac{(n^2-m^2)(\overline{n-1}^2-m^2)}{(4n^2-1)(4.\overline{n-1}^2-1)} = \mu_r \text{ if } n=m+2r, \text{ and } =\mu s \text{ if } n=m+2s+1$$

$$\frac{2n^2+2n-2m^2-1}{(2n-1)(2n+3)}.\epsilon+n(n+1)-z=\phi_r \text{ if } n=m+2r, \text{ and } =\phi s \text{ if } n=m+2s+1,$$

we have the systems

(1) $n=m+2r$, $\mu_1a_1=\frac{1}{\epsilon}\phi_0a_0$, $\mu_2a_2=\frac{1}{\epsilon}\phi_1a_1-a_0$, $\ldots$ $\mu_{r+1}a_{r+1}=\frac{1}{\epsilon}\phi_ra_r-a_{r-1}$,

z one of the roots of $a_\infty=0$.

(2) $n=m+2s+1$, $\mu_1b_1=\frac{1}{\epsilon}\phi_0b_0$, $\mu_2b_2=\frac{1}{\epsilon}\phi_1b_1-b_0$, $\ldots$ $\mu_{r+1}b_{r+1}=\frac{1}{\epsilon}\phi_rb_r-b_{r-1}$, $\ldots$

z one of the roots of $b_\infty=0$;

the expressions dividing themselves into two classes, for the former of which the values of θ are symmetrically equal, and for the latter equal and opposite, on opposite sides of the equator.

The values of Ω also fall into two corresponding classes, and are expressed most appropriately in terms of the function

$$S_n(x)=x^n\left(\frac{1}{x}\frac{d}{dx}\right)^n\frac{\sin x}{x}.$$

If we write $\xi=\lambda c\sinh\alpha$, $\eta=\lambda c\cosh\alpha$, $\zeta=\cosh\alpha$, we find for Ω these expressions:—

Class I. Any one of the three forms

$$a_0S_m(\xi)+\sum_1^\infty(-1)^ra_r\frac{1\cdot3\ldots(2r-1)}{(2m+2r+1)(2m+2r+3)\ldots(2m+4r-1)}\cdot S_{m+2r}(\xi),$$

$$\frac{(\eta^2-\lambda^2c^2)^{\frac{m}{2}}}{\eta^m}\left(a_0S_m(\eta)+\sum_1^\infty a_r\cdot\frac{2^r\cdot(m+1)(m+2)\ldots(m+r)}{(2m+2r+1)(2m+2r+3)\ldots(2m+4r-1)}S_{m+2r}(\eta)\right)$$

$$a_0P_m^m(\zeta)+\sum_1^\infty(-1)^ra_rP_m^{m+2r}(\zeta).$$

Class II. Any one of the forms

$$\frac{(\xi^2+\lambda^2c^2)^{\frac{1}{2}}}{\xi}$$

$$\left(b_0 S_{m+1}(\xi)+\sum_1^\infty (-1)^s b_s \cdot \frac{1\,.\,3\,.\,5\,\ldots\,(2s+1)}{(2m+2s+3)(2m+2s+5)\,\ldots\,(2m+4s+1)} \cdot S_{m+2s+1}(\xi)\right)$$

$$\frac{(\eta^2-\lambda^2c^2)^{\frac{m}{2}}}{\eta^m}$$

$$\left(b_0 S_{m+1}(\eta)+\sum_1^\infty b_s \cdot \frac{2^s\,.\,(m+1)(m+2)\,\ldots\,(m+s)}{(2m+2s+3)(2m+2s+5)\,\ldots\,(2m+4s+1)} \cdot S_{m+2s+1}(\eta)\right),$$

$$b_0 P_m^{m+1}(\zeta)+\sum_1^\infty (-1)^s b_s P_m^{m+2s+1}(\zeta).$$

It is shown that the roots of $a_\infty=0$ are all real and definite in position, and that in the neighbourhood of these roots the series a_r, a_{r+1}, ... rapidly converge; similar results hold for the roots of $b_\infty=0$.

The general form of the ratios $a_r : a_0$ is ascertained, and the equation $a_\infty=0$ written in the form

$$1-\epsilon^2\Sigma_1+\epsilon^4\Sigma_2-\ \ldots\ =0.$$

where Σ_r denotes the sum of the products of every r of the series $\frac{\mu_0}{\phi_0\phi_1}, \frac{\mu_1}{\phi_1\phi_2}$ ... of which no two in the same product are adjacent.

We then approximate to the values of z by series ascending by powers of ϵ, and the expression for the $\overline{r+1}$th root is, up to ϵ^5, given by

$$\phi_r=\epsilon^2\left(\frac{\mu_r}{\phi''_{r-1}}+\frac{\mu_{r+1}}{\phi''_{r+1}}\right)+\epsilon^4\left(\frac{\mu_{r-1}\mu_r}{\phi'_{r-2}\phi'^2_{r-1}}+\frac{\mu_{r+1}\mu_{r+2}}{\phi'^2_{r+1}\phi'_{r+2}}\right),$$

in which ϕ'_p represents the result of putting in ϕ_p a first approximation to z, up to ϵ given by $\phi_r=0$, and ϕ_p'' the resulting of substituting a second approximation up to ϵ^3 given by $\phi_r=\epsilon^2\left(\frac{\mu_r}{\phi'_{r-1}}+\frac{\mu_{r+1}}{\phi'_{r+1}}\right)$.

These expressions are then used to expand z in powers of ϵ; and, more especially, the first roots of each equation for any given value of m, are found. For the $\overline{r+1}$th root the term a_r is the leading term of the series, and expansions are found for $a_{r+1} : a_r$, $a_{r-1} : a_r$, $a_{r+2} : a_r$ .... as far as ϵ^3.

In the case of Class I a few of the lower roots are found for the smaller numbers, $m=0$, 1, 2, and the numerical expansions given for one or two of the coefficients adjacent to the leading one to (in general) two terms. In Class II the numerical calculations have been performed for a few of the roots only. These expansions represent the solution found by M. Mathieu.

The values of λ must be determined from the condition satisfied at the surface of the solid. If the surface be maintained at constant

(zero) temperature, λ is given either by $0=\Omega^z_m(\lambda b)$ or $0=\Omega^z_m(\lambda a)$, $2a$ and $2b$ being the major and minor axes of the ellipsoid. The latter is the more convenient for expressing λ in powers of the eccentricity $e=\frac{c}{a}$. It is shown how the values of λ may be found, and in particular for the first root of $m=0$, the equation becomes $\Omega^z_0=0$, which gives for λ,

$$\lambda a=i\pi+\frac{2}{3}i\pi e^2+\frac{i\pi}{135}\left(\frac{122}{7}i^2\pi^2+613\right)e^4+\ldots$$

When the solid cools by radiation the boundary condition is

$$\frac{dV}{d\xi}+\frac{h}{\lambda}\sqrt{1-e^2\xi^2}V=0(\xi=\lambda b);$$

and, to deal with this condition, it is necessary to show that any function of ν, at least every function capable of expansion in the form $\sum_m^\infty A_n P^n_m(\nu)$, may be expanded in the form $\Sigma B_n\theta^z_m$. The possibility of this depends on the theorem $\int_{-1}^{+1}\theta^z_m\theta^{z'}_m d\nu=0$, z not $=z'$, which is proved true.

In particular the coefficients of the expansion of $\nu^2\theta_m$ in this form are fully worked out.

To satisfy all the conditions of the problem we must now assume for V an expression of the form (cos $m\phi$ or sin $m\phi$) $e^{-\lambda^2 kt}\Sigma C_r\theta^{zr}_m\Omega^{zr}_m$; and general equations are given whereby both the values of λ and the coefficients (C) are determined. If we require the expansion of λ only as far as e^4, the equation which finds it can be readily given. If we wish only λ up to e^2, this equation breaks up into a system of the form

$$\frac{d\Omega^{zr}_m}{d\xi}+g^r_r\Omega^{zr}_m=0,\ (\xi=\lambda b),$$

in which g^r_r are constant coefficients whose values are found. The course of the subsequent approximation is free from difficulty.

To calculate the coefficients which depend on the initial state of the ellipsoid, we have to evaluate the integral

$$\int_{-1}^{+1}d\nu\int_0^{e^{-1}}v^2(\zeta^2-\nu^2)d\zeta,$$

in which v may be either $\theta^z_m\Omega^z_m$ or $\Sigma Cr\theta^{zr}_m\Omega^{zr}_m$, as the case may be; and to do so we must determine the following single integrals:—

$$\int_{-1}^{+1}(\theta^z_m)^2d\nu,\quad \int_{-1}^{+1}\nu^2\theta^z_m\theta^{z'}_m d\nu,\quad z'=\text{or not}=z,$$

$$\int_0^{e^{-1}}\Omega^z_m\Omega^{z'}_m d\zeta,\quad \int_0^{e^{-1}}\zeta^2\Omega^z_m\Omega^{z'}_m d\zeta,\quad z'=\text{or not}=z.$$

These integrals are given in the first three cases, and the formula for finding the last, which is somewhat complicated, furnished.

Finally, it is shown that the results may be transferred to the case of a planetary ellipsoid by changing, in the formulæ, ϵ into $-\epsilon$.

II. "On a New Method of Investigating the Magnetic Lines of Force in Magnets, demonstrating the Obliquity of the Equator and Axis of Bar Magnets." By Richard C. Shettle, M.D. Communicated by Dr. Royston-Pigott, F.R.S. Received April 24, 1879.

It was not until some thousand observations had been made in the manner about to be described, that I was rewarded with the discovery of the obliquity of the bar magnetic equator, which is the subject of the present communication.

On a former occasion the Royal Society did me the honour of accepting a similar research on the magnetic condition of arterial and venous blood.

My present object is to detail the manner in which I have been led up to the result now obtained.

It appeared to me that the usual method of displaying magnetic currents by means of the curves assumed by steel filings were only rough approximations.

Long after I had been occupied with these observations I became aware, through the kindness of Professor Stokes, Secretary to the Royal Society, that Sir George Airy, the Astronomer Royal, had in a short paper in the "Transactions," investigated (with a pocket compass furnished with a magnetic inch-needle) these lines of force. The method which I have adopted is essentially different, and greatly exceeds in delicacy.

1*st.*—*Inch magnetized steel needles were suspended and nicely balanced on a single silk fibre* 6 *inches long.*

These needles carried over the magnets assumed rapidly changing positions, and readily demonstrated, as in the paper alluded to, the varying force and direction as usually portrayed in treatises on magnetism. But these long needles utterly failed in the niceties of research on which I was employed.

2*nd.*—*Needles varying from half an inch to the* 1–16*th of an inch were mounted and suspended in a similar manner.*

I found the latter thoroughly competent to trace sudden changes in



FIG. 1.



FIG. 3.



FIG. 2.

the magnetic forces. And, for the purpose of recording the curves so traced out, I covered the surfaces of some bar-magnets with paper, and then ruled lines on them 1–16th of an inch apart, longitudinally as well as transversely.

The squares so formed gave exact co-ordinates for reference.

Next, the position of the suspended needle 1–16th of an inch long, or 1–8th if the curves displayed no sudden curvature, was carefully plotted in each of these squares by means of a hand lens. The accumulation of all these plottings, on careful continuation, gave the curves, as traced from pole to pole, shown in fig. 1.

The equatorial regions adjacent to the centre of the magnets examined were plotted by dotting down the exact position of the suspended minim magnet in its ultimate state of stable equilibrium; these dots were then assiduously joined by a continuous line.

In selecting the initial positions of the extraneous curves, equidistant lines were drawn parallel to the bar, and points were simultaneously elected on two equidistant lines from which to start equatorial curves, such as *a* and *b* in the fig. 3.

In every case the points of parallelism of the test needles in these equidistant lines upon being joined by a straight line P P′, exhibited *obliquity*.

But the principal obliquities are demonstrated by joining the points of contact with the bar of the curves fully traced out from these equidistant points, such as P P′.

These curves may be described *as those whose tangents are parallel to the axis of the* bar N S, at points equidistant from it (fig. 2).

According to the curvature, changing slowly or rapidly, a different set of needles is employed, which are preserved, ready for use, in glass tubes.

The work is one requiring great steadiness of manipulation. Some thousands of observations require plotting on a minute scale. Indeed, innumerable curves of great extent and variety have been plotted upon the surface of magnetic bars covered with paper, as seen in fig. 1.

Not only were these curves traced all over the surface of the magnets, but they were also traced a considerable distance away from them laterally.

The nature of these extraneous or lateral curves appears graphically hyperbolic. Any given curve thus formed is the position of the magnetic needle traced from point to point, which needle is necessarily under the influence of both the terrestrial and the bar magnetism for the time being, and its direction is *a resultant of the various forces* bringing it to a state of rest at the various points examined.

One of the most striking results of the investigation, when the curves are placed very near the centre of the bar, is the *general obliquity of the magnetic equator of the bar* (fig. 2).

This obliquity of the magnetic equator of the bar is demonstrated by the *coup d'œil* of the curves traced out magnetically, as shown in fig. 1.

The magnetic lines are the outcome of some thousand observations.*

The oblique convergency of the magnetic lines towards the north and south poles is strongly pronounced, and a neutral zone was discovered, which is indicated in fig. 1 by crosses + +.

The region of this neutral zone was explored by examining all those points where the suspended magnetic particle of steel exhibited indifference, either of its poles being observed to be equally attracted.

This interesting experiment can be far better appreciated by actual observation than by description, and is best shown perhaps by carrying the 1-4th inch test-magnet from the north towards the south pole across the equator. When over the north side of the equator the south pole dips; if it be then allowed to fall upon the bar, and is raised again, the south pole will leave the bar last. When it has been carried south across the equator the north pole dips, but if it be again allowed to fall upon the bar, and is once more raised, the south pole will leave the bar last, just as it did on the north side of the equator.

But this novel feature of an oblique zone is more beautifully displayed by examining small curves, traced out in the way indicated, by the smallest test-magnets near the geometrical centre of the bar, for lines equidistant from its axis.

The obliquity (measured as close as possible to the bar by means of curves traced out so as to enter the two sides of the bar) has a very equable value, varying according to the nature of the magnet, as regards the intensity of the magnetism, the quality of the material, and its temper, from 7° to 10°; and this result is confirmed by the investigation of the internal curves of many thousand observations on magnets from $7\frac{1}{2}$ to 18 inches long. Indeed, it was the invariable inclination of the surface lines which suggested the equatorial obliquity.

* The principle of squares was adopted in the enlargement: a lens being employed on the original chart.

III. "Preliminary Report to the Committee on Solar Physics on a Method of Detecting the Unknown Inequalities of a Series of Observations." By BALFOUR STEWART, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and WILLIAM DODGSON. Communicated to the Royal Society at the request of the Committee. Received May 15, 1879.

1. Our chief reason for suspecting the existence of a connexion between the state of the solar surface (as this is revealed by spots) and the magnetism and meteorology of the earth is derived from the fact that our observational series of sun-spots, on the one hand, and of magnetical and meteorological changes, on the other, are believed to be all subject to a common inequality, whose period (about 11 years) is virtually the same in all. But as it is only of late years that observations of great accuracy have been made in these three branches of inquiry, it is impossible to compare together more than a few series of long-period inequality, and hence some observers are still inclined to doubt the reality of a true connexion between the sun and the earth of the kind above mentioned. We are thus led to ask ourselves whether there may not be other inequalities of shorter period in these various observations, and whether we cannot devise some means of ascertaining the exact periodical times of these as well as their other properties.

We might thus expect to decide the question regarding a connexion between these three branches, for if solar observations and those of terrestrial magnetism and meteorology all exhibit a series of inequalities that are essentially the same in each, it is impossible to call in question the reality of some connexion between them.

2. The researches of Broun, Hornstein, Baxendell, and others have indicated the probable existence of inequalities in magnetism and meteorology, with periods of comparatively short length. Messrs. De La Rue, Stewart, and Loewy have likewise observed indications of a short-period fluctuation in sun-spots; but we are not aware that any attempt has yet been made to ascertain with great precision the exact period or periods of unknown inequalities either in terrestrial or in sun-spot observations.

As our sun-spot records have not yet been rendered sufficiently complete, it may be questioned whether the time has yet arrived for attempting any such investigation with respect to them; but our records of observations in magnetism and meteorology are now so long-continued and complete as to render it desirable in their case to make the attempt. This we have done in the following preliminary investigation.

3. We began first of all by making use of the records of the Kew

Observatory, which one of us had received through the kindness of the Kew Committee. In these the daily ranges of the magnetic declination are given after excluding disturbed observations by the process of Sir E. Sabine. The daily ranges are given in inches, and they denote the differences between the greatest and least values of each day's hourly tabulations from the curve, disturbances (as already mentioned) being excluded. These records extend from the beginning of 1858 to the end of 1873, embracing in all 16 years' observations.

4. Let us now proceed to describe the method which we pursued in our search for the natural inequalities of magnetic declination. We may illustrate this by a well-known case. Suppose that we had in our possession extensive records of the temperature of the earth's atmosphere at some one place in middle latitudes, and that, independently of astronomical knowledge, we were to make use of these for the purpose of investigating the natural inequalities of terrestrial temperature. We should begin by grouping the observations according to various periods taken, say, at small but definite time-intervals from each other. Now, if our series of observations were sufficiently extensive, and if some one of our various groupings together of this series should correspond to a real inequality, we should expect it to exhibit a well-defined and prominent fluctuation, whose departures above and below the mean should be of considerable amount. Suppose, for instance, that we have 24 points in our series, and that we group a long series of temperature observations in rows of 24 each, the time-distance between two contiguous members of one row being one hour. The series would thus represent the mean solar day, and we should no doubt obtain from a final summation of our rows a result exhibiting a prominent temperature fluctuation of a well-defined character, which we might measure by simply adding together all the departures of its various points from the mean, whether these points lie above or below; in fine, by obtaining the area of the curve which is the graphical representation of the inequality above and below the line of abscissæ taken to represent the mean of all the points. Suppose next that, still keeping to rows of 24, we should make the time interval between two contiguous members of a row somewhat different from one hour, whether greater or less, we should now in either case obtain a result exhibiting, when measured as above, a much smaller inequality than that given when the interval was exactly one hour, and it is even possible that if our series of observations were sufficiently extensive, we should obtain hardly any traces of an inequality whatever. In fine, when each row accurately represented a solar day the result would give an inequality of large amount, but when each row represented a period either slightly less or greater than a day the result would be an inequality of small amount.

5. We should by this process, after bestowing enormous labour in

grouping according to a great number of periods taken at small intervals from each other obtain definite results. These results might be graphically represented in the following manner: the line of abscissæ might be taken to denote the exact values of the various periods, forming a time scale in fact; while the ordinates might represent the final observed inequalities found by employing these various periods. There would thus be in the case now used for illustration a very prominent peak, corresponding to 24 hours, which would fall off rapidly on either side.

In this particular instance, having obtained as a result a period of exactly 24 hours, there would probably be no occasion to do anything more, because we have no reason to suppose the existence of any other temperature period very near 24 hours in addition to the one exactly corresponding thereto. We might, therefore, proceed finally to evaluate the obtained inequality, which would represent the mean daily variation of temperature.

6. It would be different, however, should there prove to be a number of inequalities having periods very close to one another on the time-scale. In this case, even when we had obtained a graphical representation of our results in the manner just now mentioned, it might be supposed that the various inequalities to some extent interfered with each other, affecting not only the position in the time-scale of the points of maximum inequality, but also the extent of range and the form of these inequalities. We should, therefore, have next to attempt to eliminate the effect of one inequality upon another.

The whole process would thus consist of two parts. In the first place, by enormous labour, we should have to obtain a graphical result showing the exact positions in the time-scale of the points of observed maximum inequality. Secondly, we should have to eliminate the effect of the various inequalities upon each other, provided it be found that there are several such inequalities very close together. In the present preliminary report we exhibit a method by which the great labour of the first of these two processes is materially abridged.

We have not, however, as yet advanced sufficiently far in our accurate estimation of observed maximum periods to proceed to the elimination of the effect of the various inequalities upon each other.

7. In testing our method, we began by grouping the Kew declination ranges in such a manner as to represent a period of 24·25 days. It is unnecessary to describe the details of the method by which a series of daily observations may be grouped so as to represent a period that is not an exact number of days; suffice it to say that we at length obtained a long series of upwards of 240 rows, each embracing 24 horizontal figures. Nor need we give the reasons which induced us to select the precise period of 24·25 days, since for all practical purposes this may be regarded as a period chosen at random.

Having grouped the whole 16 years' observations according to this period, we next broke up these into yearly sets. Each of these sets might thus be expected to be freed from the influence of the well-known annual inequality of declination range. These yearly sets embraced generally fifteen, but sometimes sixteen rows of twenty-four each. Our *starting-point or epoch* in all these calculations was January 1, 1858, that is to say, in all our groupings the declination range for January 1, 1858, formed the first member of a horizontal series.

The next operation was to sum up these 15 or 16 sets for each of the 24 horizontal figures. It might naturally be supposed that we should then divide each of the sums so obtained by 15 or 16, as the case might be, and then find the difference of each of the 24 quotients from the mean of all the quotients, such differences when placed together representing the inequality for that year.

There appears to be, however, reason to believe that (leaving aside all speculation as to causes) on those occasions when the daily range of the declination magnet is greatest, the variations of this daily range are greatest likewise, and possibly in nearly the same proportion (see paper by Balfour Stewart on "The Variations of the Diurnal Range of Magnetic Declination as recorded at the Prague Observatory," "Proc. Roy. Soc.," May 2, 1878). We have thus considered it as on the whole safest and best to adopt this plan with respect to our yearly results; that is to say, we have regarded the mean of the 24 points for each year as equal to 1,000, and we have represented each individual point upon this proportional scale.

8. The yearly inequality is, therefore, represented by the series made up of the differences of each of the 24 points from this normal value. As it is desirable in carrying out our programme to represent each yearly inequality by means of a curve, we have to some extent smoothed or equalised this series of differences. The primary series (A) has been converted into another series (B), also of 24 points, each point of (B) being the mean of four consecutive points of (A), and the series (B) has then been converted by a similar process into a series (C), each point of which is a mean of four consecutive points of (B). This amount of equalisation, by getting rid of what we may term accidental fluctuation is quite sufficient to enable us to draw a curve, well representing each yearly inequality. The equalised yearly inequalities corresponding to the period 24·25 days are represented in Table I.

Table I.—Equalised Yearly Inequalities of Kew Declination-range corresponding to period 24·25 days.

Year	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	−38	−38	−35	−21	−15	0	+13	+23	+40	+44	+50	+41	+31	+21	+ 3	− 1	−11	−10	0	0	− 7	−18	−31	−41
1859	−20	−24	−27	−14	− 2	+ 5	+ 6	+ 2	+ 4	+ 8	+22	+26	+23	+16	+ 7	+ 1	− 1	− 2	− 3	− 3	− 4	− 3	− 5	−12
1860	+34	+24	+ 6	−10	−27	−28	−20	−24	−22	−29	−31	−22	−19	− 8	+ 3	+ 7	+19	+22	+20	+21	+16	+18	+22	+28
1861	+25	+25	+25	+25	+28	+30	+33	+29	+26	+13	− 3	−19	−36	−46	−53	−50	−42	−34	−20	−12	0	+12	+18	+26
1862	+ 9	+13	+18	+24	+22	+14	+ 4	− 5	−17	−20	−19	−20	− 7	− 4	− 5	− 4	−10	− 7	− 7	− 1	+ 3	+ 4	+ 9	+ 6
1863	+10	+ 2	− 7	−18	−28	−35	−34	−26	−15	+ 5	+17	+21	+19	+ 7	− 2	− 6	− 5	− 3	+ 4	+11	+20	+24	+21	+18
1864	− 1	−12	−15	− 9	+ 9	+21	+18	+11	+ 5	+ 6	+13	+23	+26	+26	+20	+10	+ 2	−14	−29	−40	−42	−26	− 7	+ 5
1865	−11	− 7	− 2	− 7	− 3	− 4	+ 1	+13	+11	+ 1	−14	−18	−11	0	+ 9	− 1	− 7	− 7	+ 2	+15	+21	+17	+ 5	− 3
1866	−17	− 8	+ 3	+26	+35	+32	+28	+ 9	− 1	− 1	+ 2	+12	+20	+15	− 3	−22	−43	−46	−32	−15	+ 6	+ 9	+ 2	−11
1867	+14	+25	+29	+33	+28	+24	+22	+ 9	0	− 9	−22	−20	−24	−26	−16	−14	− 4	+ 5	+ 2	− 4	−14	−19	−15	− 4
1868	+24	+24	+21	+22	+18	+17	+ 6	−10	−20	−28	−22	−13	−10	− 9	− 6	− 3	+ 8	+ 6	− 7	−14	−19	−11	+ 8	+18
1869	+28	+28	+24	+19	+ 3	−13	−23	−36	−37	−33	−30	−24	−20	−17	−14	− 8	0	+ 6	+12	+22	+25	+30	+31	+27
1870	− 2	− 7	−10	− 9	−14	−17	−26	−29	−31	−32	−21	−14	− 5	+ 4	+16	+24	+32	+37	+30	+28	+20	+15	+11	0
1871	− 4	−13	−17	−14	+ 1	+21	+34	+44	+47	+42	+34	+20	+ 4	− 6	−13	−18	−17	−25	−33	−31	−31	−19	− 5	− 1
1872	+12	− 1	− 9	−13	−18	−17	−18	−16	−12	−10	− 5	−10	− 6	0	+ 7	+25	+27	+20	+12	+ 1	− 3	+ 8	+13	+13
1873	+27	+14	+ 6	− 2	− 3	− 2	0	+ 8	+17	+24	+24	+13	0	−17	−34	−39	−46	−40	−27	−16	+ 4	+21	+32	+36
	+90	+45	+10	+32	+34	+48	+44	+ 2	− 5	−19	− 5.	− 4	−15	−44	−81	−99	−98	−92	−76	−38	− 5	+62	+109 (sum=1162)	+105

9. A glance at the sums of this table for the whole 16 years will suffice to show that 24·25 days does not correspond to the exact period of any marked inequality. The sums are small, and we conclude therefore that we have not been fortunate in our chance selection of a period to begin with; but the peculiarity of our method is, that it will enable us to ascertain the true position in the time-scale of the neighbouring prominent inequalities by means of the results of Table I. The method of doing this can easily be rendered evident. Each horizontal row of Table I consists of 24 numbers, and there are 16 years, beginning with 1858. We may, therefore, call the numbers of the first row $(0)_{58}$, $(1)_{58}$, $(2)_{58}$, &c., $(23)_{58}$; those of the second row $(0)_{59}$, $(1)_{59}$, $(2)_{59}$, &c., $(23)_{59}$, and so on for each row.

In this table, therefore, each vertical column consists of similar numbers for the various years, adopting the notation now mentioned.

Suppose, however, that we displace these values as follows:—

1858	$(0)_{58}$,	$(1)_{58}$,	$(2)_{58}$	. .	$(21)_{58}$,	$(22)_{58}$,	$(23)_{58}$.
1859	$(1)_{59}$,	$(2)_{59}$,	$(3)_{59}$	. .	$(22)_{59}$,	$(23)_{59}$,	$(0)_{59}$.
1860	$(2)_{60}$,	$(3)_{60}$,	$(4)_{60}$	. .	$(23)_{60}$,	$(0)_{60}$,	$(1)_{60}$.
.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.
1873	$(15)_{73}$,	$(16)_{73}$,	$(17)_{73}$	. .	$(12)_{73}$,	$(13)_{73}$,	$(14)_{73}$.

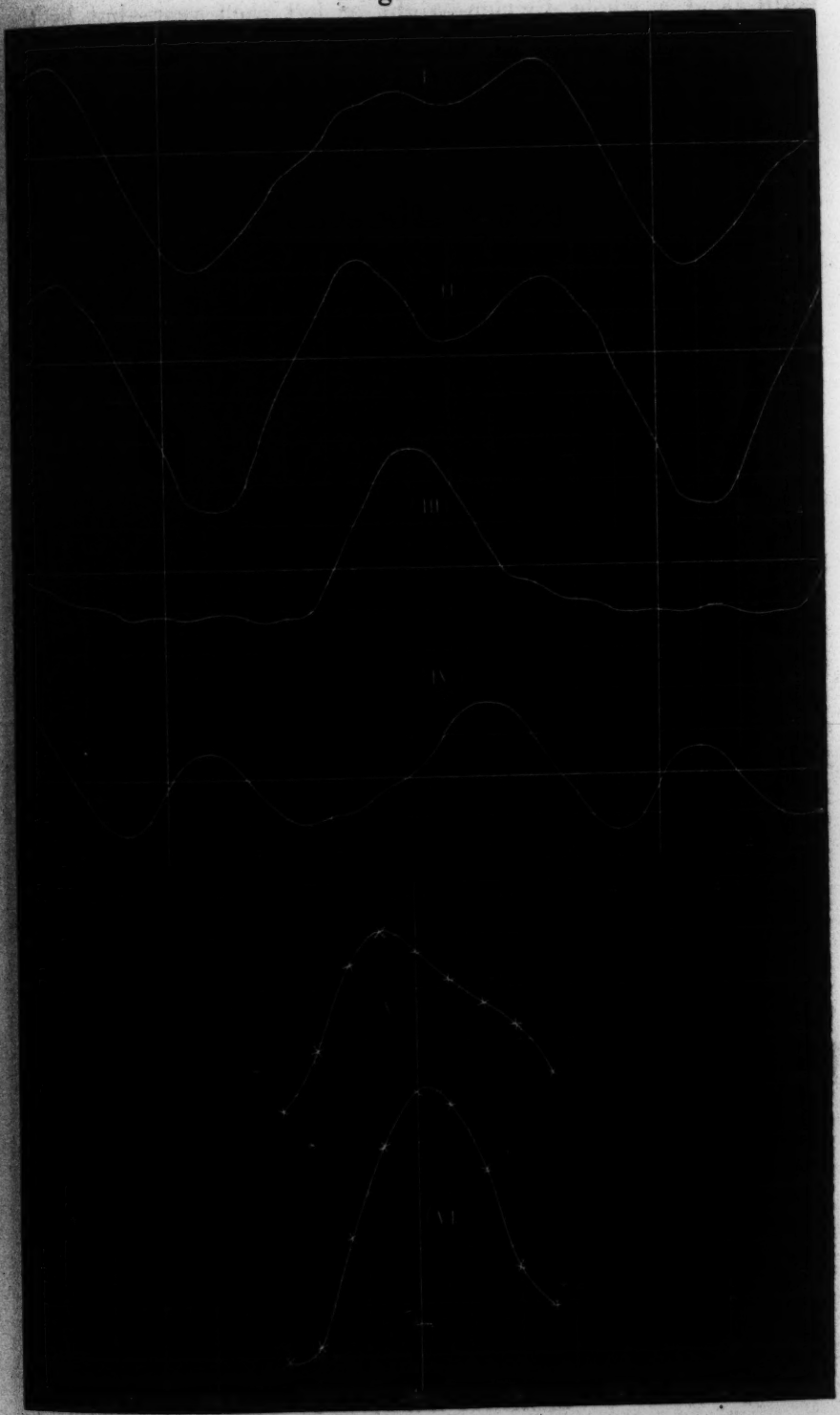
Now, if we add up the various vertical columns of this series the sums will represent an inequality somewhat larger in period than 24·25 days. For it is manifest that if we have a regular series of waves whose values we plot numerically after the manner of Table I, the consequence of adopting too small a time scale will be to throw any salient point of the wave, such as the crest, always further and further to the right, and to correct this we should have to pull the whole series a little to the left each time. Now, this is precisely what we have done in the above process, which will thus give us the representation of an inequality of larger period than 24·25 days. It is easy to find the exact length of period which the above series represents. We pull everything to the left nearly one day, but more accurately the 24th part of 24·25 days in one year. If, therefore, 365·25 days give $\frac{24\cdot25}{24}$, what will 24·25 days give? We find from this proportion that the period of the inequality indicated by performing the above process is $24\cdot25+\frac{(24\cdot25)^2}{24\times365\cdot25}=24\cdot317$ days. Again, we may pull things to the left two, three, or four divisions each year, and thus obtain the representation of inequalities with periods of 24·384, 24·451, or 24·518 days.

Or we may perform the opposite operation of pushing things to the right one division each year, and thus obtain the representation of an inequality, having a period of 24·183 days, while 2, 3, or 4 such divisions each year would give us periods of 24·116, 24·049, or 23·982 days.



Diagram 1.

Diagram II.



10. We have found it necessary to push things not merely by the multiple of a whole division—right or left each year—but by the multiple of half a division.

To accomplish this we must obtain for every alternate year a series of half-way points, which is best done by converting the series of such years into curves, and using these to give us the half-way points.

For instance, should we wish to pull things to the left half a division each year, we should adopt the following arrangement.—

1858 . .	$(0\frac{1}{2})_{58}$,	$(1\frac{1}{2})_{58}$,	$(2\frac{1}{2})_{58}$	. .	$(21\frac{1}{2})_{58}$,	$(22\frac{1}{2})_{58}$,	$(23\frac{1}{2})_{58}$.
1859 . .	$(1)_{59}$,	$(2)_{59}$,	$(3)_{59}$	. .	$(22)_{59}$,	$(23)_{59}$,	$(0)_{59}$.
1860 . .	$(1\frac{1}{2})_{60}$,	$(2\frac{1}{2})_{60}$,	$(3\frac{1}{2})_{60}$	. .	$(22\frac{1}{2})_{60}$,	$(23\frac{1}{2}{}_{60})$,	$(0\frac{1}{2})_{60}$.
1861 . .	$(2)_{61}$,	$(3)_{61}$,	$(4)_{61}$	. .	$(23)_{61}$,	$(0)_{61}$,	$(1)_{61}$.
. .	.	.	.	.	.	.	.	.

A further extension of the same method would enable us to go from halves to quarters of a division, but we have not yet met with any inequality that is not sufficiently well indicated by half divisions.

11. We have found the following plan very useful in abridging the labour of these reductions.

Sixteen perfectly similar strips of thick paper are taken, one for each year. Each of these strips is divided into 48 small compartments, a vertical black line being ruled at the beginning, the middle, and the end. Upon the first of these strips the inequality, say for 1858, is written in duplicate, so that the second 24 figures are a repetition of the first. The same is done for the other years. These strips are then attached to a frame which allows them to slide along with regard to each other, and at the same time only exposes 24 lines of figures at a time. By this method they can easily be arranged according to any given order, and the sums made with very little trouble.

It is further desirable to exhibit all the positive values, say in black, and all the negative values in red, so that the eye may easily distinguish between them.

The following will exhibit the nature of the arrangement—only to save space we take an inequality consisting of only four terms.

$(0)_{58}$	$(1)_{58}$	$(2)_{58}$	$(3)_{58}$	$(0)_{58}$	$(1)_{58}$	$(2)_{58}$	$(3)_{58}$
$(0)_{59}$	$(1)_{59}$	$(2)_{59}$	$(3)_{59}$	$(0)_{59}$	$(1)_{59}$	$(2)_{59}$	$(3)_{59}$
$(0)_{60}$	$(1)_{60}$	$(2)_{60}$	$(3)_{60}$	$(0)_{60}$	$(1)_{60}$	$(2)_{60}$	$(3)_{60}$
$(0)_{61}$	$(1)_{61}$	$(2)_{61}$	$(3)_{61}$	$(0)_{61}$	$(1)_{61}$	$(2)_{61}$	$(3)_{61}$

The vertical portion between the brackets is here all that is exposed.

12. Before concluding this description of our method we ought to remark that it can only be considered as correct for periods not far removed on either side from that for which the series or book (as we call it) was originally framed. Thus, in the present instance, while the results of Table I will readily indicate inequalities that lie six divisions (of 0·067 day each) either to the right or left of the normal series, it is apparent that +12 divisions from the normal must necessarily exhibit the same result as −12; it will not therefore do to push the method nearly so far.

13. When this method is applied to Table I we obtain the following results:—

Table II.—Exhibiting the results of the above method applied to the numbers of Table I.

Divisions from normal.	Exact period in days.	Magnitude of inequality.
−8·0	23·7133	1438
−7·5	23·7469	1757
−7·0	23·7804	1546
−6·5	23·8140	1047
−6·0	23·8475	1707
−5·5	23·8810	1141
−5·0	23·9146	1002
−4·5	23·9481	2009
−4·0	23·9817	2954
−3·5	24·0152	3109
−3·0	24·0487	2504
−2·5	24·0823	1289
−2·0	24·1158	1068
−1·5	24·1494	1473
−1·0	24·1829	1700
−0·5	24·2165	1513
Normal	24·2500	1162
+0·5	24·2835	1115
+1·0	24·3171	1398
+1·5	24·3506	867
+2·0	24·384[illegible]	1062

14. The results of Table II are exhibited graphically by means of a curve in Diagram I, fig. I, which accompanies this paper.

In this curve the abscissæ denote periods, while the ordinates represent corresponding inequalities. It will be noticed that we have

Table III.—Equalised Yearly Inequalities of Kew Declination-range corresponding to period 24·00 days.

Year	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	−34	−48	−51	−46	−44	−31	−15	− 1	+19	+29	+35	+36	+36	+38	+31	+21	+ 7	− 9	− 4	+ 9	+17	+21	+ 5	−20
1859	−20	−26	−22	−24	−22	−18	−21	−17	−13	− 2	+ 9	+11	+17	+12	+15	+23	+22	+28	+25	+20	+16	+ 6	− 5	−12
1860	+ 5	+14	+18	+18	+16	+17	+19	+26	+39	+44	+41	+22	− 8	−34	−50	−49	−35	−21	−10	−10	−16	−19	−17	− 6
1861	−27	−47	−56	−62	−56	−45	−36	−21	− 6	+ 8	+15	+22	+24	+21	+29	+26	+27	+32	+30	+36	+38	+30	+17	− 3
1862	−24	−30	−22	−13	− 1	+ 6	+ 5	+ 1	− 1	0	− 2	− 5	−11	−17	−13	− 2	+10	+20	+25	+28	+27	+21	+ 8	− 8
1863	−17	−17	−23	−21	−15	0	+22	+32	+33	+24	+10	+ 4	+ 6	+ 3	+ 2	− 2	− 4	+ 6	+10	+ 8	− 4	−17	−21	−21
1864	+21	+ 8	0	− 9	− 7	0	+ 5	+ 1	− 2	− 5	− 9	− 6	− 8	− 4	+ 1	+ 6	+13	+ 9	+ 2	− 8	−17	− 7	+ 2	+16
1865	+ 2	0	− 3	− 5	− 4	− 5	−10	− 9	− 9	− 2	+14	+19	+26	+21	+ 4	+ 1	− 3	+ 2	+ 6	− 6	−12	−15	−11	0
1866	−34	−33	−17	− 5	+ 6	+ 8	− 4	−16	−19	−14	− 2	+20	+26	+24	+15	0	− 2	+ 2	+10	+22	+22	+13	− 3	−24
1867	−55	−40	−26	− 5	+14	+24	+30	+19	+ 6	+ 2	0	+ 8	+21	+26	+31	+30	+28	+30	+20	+ 6	−10	−40	−51	−56
1868	−27	−29	−23	−16	−13	−14	− 8	− 2	+ 1	+ 3	−10	−19	−23	−15	+ 5	+16	+28	+29	+31	+39	+33	+25	+ 5	−17
1869	0	−17	−20	−25	−27	−33	−40	−47	−48	−36	−24	− 8	+ 6	+11	+16	+23	+29	+32	+37	+41	+41	+43	+31	+15
1870	+ 2	+ 2	− 3	0	− 8	−17	−22	−31	−34	−25	−18	− 6	+ 6	+ 9	+12	+17	+22	+25	+24	+21	+12	+ 6	+ 6	− 2
1871	−12	0	+ 2	− 2	−11	−20	−10	+ 4	+20	+41	+46	+41	+34	+23	+15	+18	+16	+ 6	− 5	−30	−48	−51	−47	−31
1872	+14	+ 6	+ 3	0	+ 3	+ 8	+11	+ 8	+ 1	− 8	−19	−24	−19	−15	−10	− 6	−10	− 7	− 4	+ 1	+11	+16	+19	+21
1873	−27	−32	−38	−34	−22	− 5	+15	+30	+39	+35	+21	+11	− 3	− 2	− 3	− 5	− 4	− 5	+ 7	+13	+16	+11	− 3	−14
	−233	−289	−281	−249	−191	−125	−59	−23	+26	+94	+107	+126	+130	+101	+100	+117	+144	+179	+204	+190	+126	+43	−65	−162
																							(sum = 3364)	

indications of several maximum points, more especially of one corresponding very nearly to 24 days.

This has induced us to make a new book for 24 days in addition to the one for 24·25 days, the construction of which we have already described. Both have been treated in precisely the same manner.

The results of this book are given in Table III.

15. When our method is applied to Table III, we obtain the following results:—

Table IV.—Exhibiting the results of the above method applied to the numbers of Table III.

Divisions from normal.	Exact period in days.	Magnitude of inequality.
−7·5	23·5072	1310
−7·0	23·5400	1280
−6·5	23·5729	2568
−6·0	23·6057	2710
−5·5	23·6386	2128
−5·0	23·6715	1070
−4·5	23·7043	674
−4·0	23·7372	1260
−3·5	23·7700	1500
−3·0	23·8029	922
−2·5	23·8357	1394
−2·0	23·8686	1976
−1·5	23·9014	1096
−1·0	23·9343	1754
−0·5	23·9671	2464
Normal	24·0000	3364
+0·5	24·0329	2960
+1·0	24·0657	1974
+1·5	24·0986	1032
+2·0	24·1314	1548
+2·5	24·1643	2174
+3·0	24·1971	2160
+3·5	24·2300	1664
+4·0	24·2628	1278
+4·5	24·2957	1570
+5·0	24·3285	1456
+5·5	24·3614	954
+6·0	24·3943	1110

16. The results of Table IV are exhibited graphically by means of a curve in Diagram I, fig. II.

It will at once be seen from this curve that it exhibits very nearly the same positions for maximum inequalities as those shown in fig. I.

Thus, by selecting at random the period 24·25 days we are by our method referred to nearly the true positions of the various maximum inequalities, and it might then be well to construct a separate book for each of these, as we have done for the large inequality corresponding to 24 days nearly.

17. While this method has succeeded in bringing before us the hitherto unknown inequalities of the Kew declination-range, it might yet be imagined that the results obtained are of a local or semi-local, and not of a truly cosmical nature. To test this we have compared together the daily declination-ranges as recorded at Trevandrum by Mr. Broun during the years 1858–64, with the corresponding portions of the Kew series. Plotting these in series of 24 days each we have obtained by the method already described the following results:—

Table V.—Comparing together by the above method 7 years of Trevandrum and 7 years of Kew Declination-range.

Divisions from normal.	Exact period in days.	Magnitude of inequality. Kew.	Trevandrum.
−2·0	23·8686	925	1235
−1·5	23·9014	1206	1301
−1·0	23·9343	1633	1809
−0·5	23·9671	1774	2245
Normal	24·0000	1687	2507
+0·5	24·0329	1546	2448
+1·0	24·0657	1425	2123
+1·5	24·0986	1322	1662
+2·0	24·1314	1085	1489

The results of Table V are exhibited graphically in Diagram II, figs. V, VI, No. V denoting the Kew, and No. VI the Trevandrum inequality.

18. We see from Table V that the maximum inequality occurs for these 7 years, both for Kew and Trevandrum, at points not far distant from 24·00 days. There are thus exhibited signs of repetition in this Kew inequality, as well as evidences of its cosmical nature, inasmuch as Trevandrum gives results similar to Kew as far as *period* is concerned.

19. We shall now compare together the *forms* of the inequalities in these two places.

Kew, 1858 to 1864.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	−219	−334	−357	−359	−295	−162	−48	+48
	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	+158	+224	+226	+192	+128	+43	+34	+53
	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
	+91	+149	+178	+190	+139	+80	−25	−123
Trevandrum, as above.	−217	−224	−215	−206	−226	−247	−233	−213
	−53	+158	+345	+526	+562	+539	+407	+240
	+91	−50	−73	−128	−158	−178	−219	−222

In the above comparison the inequalities for 7 years are multiplied by $\frac{16}{7}$ in order to bring them to the same scale with that for 16 years exhibited in Table III.

In Diagram II, which accompanies this paper, the Kew declination inequality for 24 days (16 years) is given in fig. I.

The Kew declination inequality for 24 days (7 years) is given in fig. II, while the Trevandrum declination inequality for 24 days (7 years) is exhibited in fig. III. It will be seen that there is a very considerable likeness between all the three curves which are of the same type. The Trevandrum inequality is more marked than the corresponding Kew one—this may possibly be due to the fact that in the Kew observations the disturbances were eliminated.

20. Our method has hitherto been applied to magnetic declination-ranges. But if the cause of these inequalities be cosmical and connected with our luminary, we might suppose that the meteorological elements of the earth would also be affected. We have, therefore, taken the Kew diurnal temperature ranges—in other words, the difference between the daily indications of the maximum and minimum thermometers for the same 16 years for which we have declination-ranges, and we have treated these in precisely the manner already described, that is to say, we have grouped these ranges into series of 24 days each, and dealt with them as we have dealt with the similar declination series. The results are recorded in the following table :—

Table VI.—Equalised Yearly Inequalities of Kew Temperature-range corresponding to period 24·00 days.

Year	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	−45	−17	+ 6	+15	+21	+ 9	+ 8	+18	+11	+14	0	− 1	+18	+30	+54	+50	+32	+18	− 2	−18	−38	−54	−65	−64
1859	+ 1	+ 9	+ 7	+ 7	− 6	−11	−20	−38	−37	−26	− 2	+33	+42	+22	+ 5	−12	− 5	+27	+37	+30	+ 6	−22	−26	−21
1860	+ 6	+ 3	−11	−24	−34	−32	−20	+ 4	+34	+53	+58	+49	+32	+26	+28	+19	+11	− 9	−31	−44	−48	−40	−25	− 5
1861	−71	−63	−64	−78	−90	−87	−78	−55	−35	−13	+19	+41	+61	+77	+90	+112	+114	+106	+94	+61	+22	−21	−61	−81
1862	−70	−62	−49	−37	−25	− 5	− 6	− 8	−14	−15	+17	+44	+65	+72	+58	+52	+41	+43	+47	+31	+ 4	−34	−66	−83
1863	+14	+25	+30	+36	+40	+33	+28	+37	+39	+57	+63	+39	+21	− 9	−30	−39	−54	−65	−74	−67	−59	−41	−18	− 6
1864	+65	+43	−12	−42	− 55	−52	−21	−13	−22	−36	−43	−22	+ 9	+55	+74	+57	+31	−19	−43	−39	−28	+ 8	+46	+59
1865	−109	−77	−28	+18	+51	+65	+62	+60	+60	+43	+26	− 1	−31	−27	− 2	+28	+52	+59	+38	+ 2	−23	−64	−95	−107
1866	+16	−15	−30	−32	−44	−18	+ 4	+ 3	+10	− 8	−25	−20	−17	− 4	+ 5	− 8	−12	−12	+ 5	+33	+48	+57	+40	+24
1867	+ 8	+30	+63	+94	+98	+68	+29	− 8	−36	−45	−47	−60	−69	−82	−80	−53	−29	− 6	+10	+14	+28	+35	+25	+13
1868	−10	+31	+40	+32	+29	+ 8	+ 9	+10	+ 5	+ 7	+10	+29	+45	+47	+47	+21	+ 5	− 7	−39	−55	−73	−85	−68	−38
1869	−21	− 3	+ 3	+13	+ 9	+ 3	− 6	−14	− 1	+ 8	+14	+19	+23	+31	+47	+47	+34	+11	−29	−41	−44	−50	−29	−24
1870	− 0	+ 2	−11	−26	−50	−84	−98	−99	−73	−43	−17	+ 4	+ 4	+19	+36	+49	+71	+84	+85	+73	+49	+22	+ 5	− 2
1871	+79	+101	+104	+99	+67	+50	+39	+16	− 4	−41	−86	−111	−128	−114	−82	−51	−23	− 1	+ 7	+ 7	+ 7	+ 5	+19	+41
1872	+70	+74	+64	+35	− 6	−33	−58	−65	−69	−61	−39	−28	− 4	+16	+33	+43	+34	+15	− 9	−24	−25	−12	+10	+39
1873	+26	+22	+21	− 4	− 34	−55	−65	−68	−61	−61	−68	−65	−59	−45	− 12	+25	+50	+68	+77	+77	+79	+71	+47	+34
	−41	+103	+133	+106	−29	−141	−193	−220	−193	−167	−120	− 50	+12	+114	+271	+340	+352	+312	+173	+40	−95	−225	−261	−221
																							(sum = 3912)	

21. When our method is applied to Table VI we obtain the following results :—

Table VII.—Exhibiting the results of the above method applied to the numbers of Table VI.

Divisions from normal.	Exact period in days.	Magnitude of inequality.
−7·0	23·5400	2100
−6·5	23·5729	3093
−6·0	23·6057	4700
−5·5	23·6386	4025
−5·0	23·6715	1386
−4·5	23·7043	1887
−4·0	23·7372	3910
−3·5	23·7700	3915
−3·0	23·8029	3140
−2·5	23·8357	2771
−2·0	23·8686	4234
−1·5	23·9014	5921
−1·0	23·9343	5518
−0·5	23·9671	2374
Normal	24·0000	3912
+0·5	24·0329	5135
+1·0	24·0657	4516
+1·5	24·0986	2157
+2·0	24·1314	2378
+2·5	24·1643	3795
+3·0	24·1971	3926
+3·5	24·2300	3043
+4·0	24·2628	2520
+4·5	24·2957	3004
+5·0	24·3285	4302
+5·5	24·3614	4761
+6·0	24·3943	5824

The results of the above table are graphically represented by a curve in Diagram I, fig. III. It will be seen that there is a great, though not absolutely exact, coincidence between the periods recorded in this curve, and those of the same diagram which exhibit the declination results.

The form of the temperature inequality for 24 days, *see* Table VI, is exhibited in Diagram II, fig. IV. It will be seen that there is a considerable likeness in form between the various curves of this diagram; but it must not be forgotten that these curves are in all probability affected by the influence of adjacent inequalities.

22. We have thus given our method a preliminary trial, and the result is we think decidedly hopeful. We are inclined to believe that when more completely worked out it may not only reduce to certainty the existence of periods of short range in terrestrial magnetism and meteorology, but also give determinations of the exact lengths of these periods, and of the forms of the inequalities.

We would remark, in conclusion, that a glance at the yearly inequalities exhibited in Tables I and III, will show us that these are more marked and more regular in those years which correspond to maximum sun-spots, than they are in years of minimum sun-spots.

Note on the above Paper. By Professor G. G. Stokes.

As the search for periodic inequalities of unknown period must always be more or less laborious, it seems desirable to point out another mode in which the search might be conducted, and which seems to offer great facilities for the object, assuming the possession of the required instrument.

It seems to me that the harmonic analyser of Sir William Thomson is singularly well adapted to this purpose, which, as I have ascertained from him, was one of the applications of his machine that he has had in view.

If $f(t)$ be any function of the time t given by observation, and $2\pi \div n$ a period p assumed at pleasure, then by plotting if necessary the function on a scale adapted to the paper cylinder of the machine, we shall get, by a simple mechanical process, the values of the integrals

$$\int f(t) \sin nt\, dt, \qquad \int f(t) \cos nt\, dt,$$

between any limits. We may take the inferior limit for the origin of the time, and then by reading off the cylinders of the machine for as many values of the superior limit t as we please, we shall get the corresponding values, as many as we like, of the integrals.

Suppose, now, that $f(t)$ contains a small term of the form

$$c \sin (n't+\alpha),$$

where n' is not much different from n, so that the period tried approaches closely to the period p' of this inequality. The corresponding part of the integrals will be—

$$\frac{c}{2(n'-n)} \sin \{(n'-n)t+\alpha\} - \frac{c}{2(n'+n)} \sin \{(n'+n)t+\alpha\},$$

and $$-\frac{c}{2(n'-n)} \cos \{(n'-n)t+\alpha\} - \frac{c}{2(n'+n)} \cos \{(n'+n)t+\alpha\},$$

taken between the proper limits. The terms divided by $n'+n$ having but a small coefficient in the numerator, and having a denominator which is not small, may be left to take their chance with casual fluctuations; but the terms divided by $n'-n$ rise into importance from the smallness of the denominator, and express an inequality in the integrals of comparatively large amount and long period.

We may therefore confine our attention to the terms

$$\frac{c}{2(n'-n)}\sin[(n'-n)t+\alpha], \qquad \frac{c}{2(n'-n)}\cos[(n'-n)t+\alpha],$$

occurring in the indefinite sine integral and cosine integral respectively.

If, therefore, the values of these two integrals, obtained from the two cylinders respectively, be plotted, we shall obtain a periodic fluctuation of a period more or less long as we hit on a period more or less near to that of the inequality which we have supposed to exist. The zero points of the fluctuation in the sine integral will correspond to the maxima and minima in that of the cosine integral, and *vice versâ*. The reciprocal of the period of the fluctuation will give the difference of the reciprocals of p and p', and thus p' will be known from p provided only we know which of the two, p, p', is the greater. This will be shown by comparing the phases of the fluctuations of the two integrals. If $n'>n$, the fluctuation of the cosine integral will be a quarter of a period behind, if $n'<n$ before, that of the sine integral.

If $f(t)$ be subject to known periodic inequalities with approximately known coefficients, the integrals should be cleared of the terms thence arising, if of sufficient moment, by using their analytical expressions, and the residues only plotted.

Of course $f(t)$ is not necessarily a function of the time given by *direct* observation; it might be a function deduced from one so obtained. For example, $f(t)$ might be the coefficient of the principal term in the daily fluctuation of the element when each day's record is separately subjected to harmonic reduction.

May 29, 1879.

IV. Researches on Explosives. No. II. (Fired Gunpowder.)." By Captain Noble, late R.A., F.R.S., F.R.A.S., F.C.S., and F. A. Abel, C.B., F.R.S., V.P.C.S. Received May 21, 1879.

(Abstract.)

The authors preface this memoir by two tables: one of these gives the results of some analyses of products of explosion of the three service powders of Waltham Abbey manufacture, pebble, R.L.G. and F.G., which are required to complete the series of results obtained by

firing the charge in different spaces, which were given in their first memoir on this subject, and a statement of the mean percentage composition by volume of the gases and by weight of the solid products furnished by those three powders, together with the highest and lowest proportions in which each product has been obtained with the three descriptions of powder. This table includes the results of examination of the products of explosion of four descriptions of powder differing in many respects from the powders chiefly employed in their researches. The other table gives the complete results of the whole series of analyses made, showing the proportion by weight of each solid and gaseous product, and including the amount of water pre-existent in the various specimens operated on.

The following abstract of the table first referred to includes the analytical results of the products furnished by the four special samples of powder, and the highest, lowest, and mean proportions of the several products furnished by the three principal powders used. (See next page.)

A portion of this memoir is devoted to a discussion of a few points raised in reference to the first memoir of the authors, on fired gunpowder, by General Morin and M. Berthelot, who were appointed by the Académie des Sciences as a Commission to report on that memoir. These points were specified and discussed by those savants in a joint report, and in two separate memoirs communicated by M. Berthelot to the Académie.

The principal points to which attention has thus been drawn are as follow :—

1. Potassium hyposulphite has been found as one of the products of combustion of gunpowder by every recent investigator. But the question arises: Is this product either wholly or in part primary? or is it to be considered as secondary, formed from the primary products during the rapid loss of heat to which they are exposed? or is it, finally, to be considered only as formed from the sulphide by the absorption of oxygen during the processes of removal from the cylinder and analysis, and therefore to be regarded as an accidental product?

2. The authors, from their investigation of the products of explosion in gunpowder in close spaces, conclude that "any attempt to express, even in a complicated chemical equation, the nature of the metamorphosis which a gunpowder of average composition may be considered to undergo, would only be calculated to convey an erroneous impression as to the simplicity or definite nature of the chemical results and their uniformity under different conditions, while possessing no important bearing upon an elucidation of the theory of the explosion of gunpowder."

M. Berthelot, in one of his memoirs based upon the authors' results, proposes to represent these results by a system of simultaneous equa-

Nature of powder.	Mean density of products of combustion.	Percentage composition by volume of the gas.							Percentage composition by weight of the solid residue.									
		Carbonic anhydride.	Carbonic oxide.	Nitrogen.	Sulphydric acid.	Marsh-gas.	Hydrogen.	Oxygen.	Potassium carbonate.	Potassium sulphate.	Potassium hyposulphite.	Potassium monosulphide.	Potassium sulphocyanate.	Potassium nitrate.	Potassium oxide.	Ammonium sesqui-carbonate.	Sulphur.	Charcoal.
Pebble W.A.	Means	48·94	13·70	32·14	2·60	0·31	2·34	...	57·17	12·46	13·89	10·25	0·27	0·23	...	0·10	5·45	0·15
	Highest	51·75	16·09	32·75	4·23	0·68	3·50	...	64·20	15·02	32·18	19·12	0·57	0·48	...	0·17	8·45	1·35
	Lowest	44·78	10·87	31·31	1·70	0·00	1·67	...	50·20	9·13	3·71	2·23	0·06	0·00	...	0·06	0·61	trace.
R.L.G. W.A.	Means	49·47	12·47	32·91	2·65	0·43	2·19	0·06	59·05	14·58	13·20	6·27	0·22	0·26	...	0·07	6·26	0·07
	Highest	52·65	17·04	35·60	4·29	0·84	3·01	0·57	66 51	24·22	25·33	9·92	0·49	0·56	...	0·18	12·03	0·71
	Lowest	46·29	8·98	30·29	1·56	0·00	1·27	0·00	48·68	4·64	3·08	2·02	0·05	0·00	...	0·04	1·25	trace.
F.G. W.A.	Means	50·62	10·46	33·20	2·48	0·19	2·96	0·08	48·31	21·11	24·79	2·41	0·12	0·16	1·29	0·05	1·75	
	Highest	53·34	16·25	34·64	3·76	0·50	4·13	0·28	59·39	24·22	34·61	5·12	0·25	0·26	5·39	0·15	5·72	
	Lowest	44·76	7·71	32·22	2·00	0·00	2·04	0·00	41·88	17·86	5·30	0·00	0·02	0·08	0·00	0·01	0·09	
Spanish Spherical	70 per cent.	53·34	4·62	37·80	2·74	...	1·29	0·21	34·97	47·62	7·60	3·17	0·04	0·93	...	0·04	5·63	
Curtis and Harvey, No. 6	30 per cent.	50·22	7·52	34·46	2·08	2·46	3·26	...	58·51	21·43	3·93	10·02	...	0·29	...	0·09	5·73	
Mining Powder	30 per cent.	32·15	33·75	19·03	7·10	2·73	5·24	...	40·78	0·58	5·84	33·20	2·92	0·09	...	1·76	12·93	2·00

tions expressing the chemical metamorphosis undergone by powder, at least as far as regards its fundamental products.

3. In directing attention to the determination by the authors of the heat disengaged by the explosion of powder (the results of which were put forward by them with some reserve), MM. Morin and Berthelot regard the figure arrived at as too low, partly because of a deficiency in delicacy of the apparatus, and partly because higher figures have been arrived at by M. Tromenec and by MM. Roux and Sarrau.

In discussing M. Berthelot's views respecting the objections which the authors raise against the acceptance of any chemical equation as giving even a general idea of the metamorphosis which a gunpowder of average composition may be considered to undergo when exploded in a confined space, they disclaim having had any intention to convey the impression, which indeed they consider that their expressions were not calculated to convey, that it was impossible to put into *some* form of equation a representation of a variety of reactions which, if assumed to take place simultaneously, among different proportions of the powder-constituents, might give approximate expression to the results obtained in any one particular experiment, and might, thus far, afford some approach to a theoretical representation of the metamorphosis of gunpowder. What they desired to point out and lay stress upon was the conclusive proof, which is afforded by the very great variations in composition, of the solid portions more particularly, of the products of explosion of samples of gunpowder presenting only small differences of composition (and even of the products furnished at different times by one and the same sample), that the reactions which occur among the powder-constituents are susceptible of very considerable variations, regarding the causes of which it appears only possible to form conjectures, and that, consequently, "no value whatever can be attached to any attempt to give a general chemical expression to the metamorphosis of gunpowder of normal composition."

Starting with the assumption (which the authors cannot admit to be a correct interpretation of their view) that they regard the variations which occur in the principal products of explosion as "opposed to all general chemical representation of the metamorphosis produced by explosion," M. Berthelot proceeds to argue that the formation of those products, viz., of carbon dioxide, carbon oxide, potassium sulphate, sulphide and carbonate from a powder of normal composition, is simply and satisfactorily accounted for by assuming the simultaneous occurrence, in variable proportions, of either *three* or *four* out of *five* different theoretical reactions, according as to whether the proportion of sulphate found is so small that it may be neglected, or amounts to 12 or 14 per cent.

After giving further equations which apply to the extreme results

(in regard to the chief products only) assumed to be attainable from the introduction, on the one hand of excess of saltpetre, on the other of excess of charcoal, into the composition of powder, M. Berthelot passes to what he terms the accessory products and, excluding from these potassium hyposulphite, which he deals with separately, he first gives *two* equations to account for the production of sulphocyanide; then *two more* to explain the existence of ammonium sesqui-carbonate (which he believes to be formed by the action of water vapour on potassium cyanide); the existence of sulphuretted or free hydrogen is explained by *two more* equations, and marsh-gas is assumed to result from "the pyrogenous decomposition of the charcoal in the powder." Lastly, an equation is given to account for the possible formation of traces of hyposulphite, which Berthelot however regards entirely as a product formed during the collection and analytical treatment of the solid residue, but which the authors nevertheless believe they now conclusively prove to be formed in very notable quantities before the solid residue can have undergone alteration from external causes.

It will be seen from the foregoing outline of M. Berthelot's theoretical explanation of the chemical changes involved in the metamorphosis of gunpowder, that the simplest form of expression which he can give to the formation of the products of explosion consists in the incorporation of *nine* or *ten* distinct reactions, occurring simultaneously but in very variable proportions, which have to be supplemented by *three* or *four other* chemical equations, by which the formation, during the process of cooling of certain products believed to be secondary, is explained. Now, although such speculations as the above are unquestionably interesting, and, it may be added, of a nature which must occur to those who desire to give some kind of definite explanation, for purposes of elementary instruction, of the chemical changes involved in the explosion of powder, the authors fail to see that, beyond this, they do more than afford the strongest confirmation of the correctness of their conclusion, that "no value whatever can be attached to any attempt to give a general chemical expression to the metamorphosis of a gunpowder of normal composition."

With reference to the potassium hyposulphite, which, in the analyses of the solid products published in the first memoir, ranged from 3 to 35 per cent. (the monosulphide being generally small in amount when the proportion of hyposulphite was high), the authors reply to M. Berthelot's suggestion, that its existence may be only ascribable to the oxidation of sulphide during the operation of removal from the explosion-vessel and of analysis, by pointing out the nature of the precautions taken to avoid oxidation of sulphide in the collection of the solid residue, and its preservation for analysis, and by admitting the formation of some hyposulphite during the unavoidable exposure to air of the residues while they were being removed

from the cylinder, especially in some instances when the structure of the residue was favourable to atmospheric action, and when the development of heat afforded proof of the occurrence of such oxidation. They point out, on the other hand, that no hyposulphite could be accidentally formed from sulphide during the analytical operations as carried out by them, and they proceed to show that the very considerable variations in the amount of hyposulphite, as well as the large amount of sulphide found in several instances, in distinct operations conducted with very great uniformity, afford substantial proof that accidental atmospheric oxidation during the collection and analysis of the residues, is not sufficient to account for all but the very small quantities of hyposulphite which M. Berthelot considers could only have pre-existed in the residue examined by the authors.

They furthermore direct attention to the following facts given in their first memoir, which appear to have been overlooked by M. Berthelot.

1. Separate examinations (conducted precisely alike) of the upper and lower portions of some of the residues, showed that considerably larger proportions of hyposulphite existed in the *upper* portions. In one case, quoted by them in their first memoir, the upper portion contained 17·14 per cent. of hyposulphite, while the lower portion only contained 4·34 per cent. At the same time there was only a difference of 1·27 per cent. in the proportions of monosulphide existing in the two portions of the residue (6·03 in the upper part and 7·3 in the lower), while there was a very great difference in the amount of free sulphur (4·88 in the upper part and 10·09 in the lower).

2. One of the small buttons of the fused solid products, of which there was generally one found attached to the firing plug in the cylinder, was examined for sulphide and hyposulphite (it having been detached without fracture and at once sealed up in a small tube). It contained the latter, but none of the former, while the mass of the residue of this particular experiment contained a somewhat considerable proportion of sulphide.

3. The production of high proportions of hyposulphite was but little affected by any variations in the circumstances attending the several explosions (*i.e.*, whether the spaces in which the powder was exploded were great or small), excepting that the amount was high in all three cases when the powder was exploded in the largest space. The analytical results given in the earlier memoir show abundantly, on the other hand, that a great reduction in the size of grain of the powder used had a great influence upon the production of hyposulphite.

Although the authors consider that the formation of hyposulphite in the solid residue, before the explosion-vessel is opened, and, indeed, to such an amount that it must be regarded as an important product

(whether primary or secondary) is conclusively demonstrated by a thorough examination into the results given in their first memoir, they have been anxious to obtain, if possible, some further decisive evidence as to the probable proportions of hyposulphite actually existing in the residues furnished by the explosion of gunpowder in closed vessels. They, therefore, introduced some variations in the methods of collecting and preparing the solid products for analysis, of which the following is an outline.

In two experiments with R.L.G. and pebble powders, the solid products, collected as rapidly as possible, were divided into two equal parts, the one consisting of the upper, the other of the lower, part of the mass. These portions were again equally divided; one-half, composed of large lumps, was as rapidly as possible (being but a few moments exposed to air) sealed up in dry bottles freed from oxygen; the other half of each was ground finely and exposed to the air for forty-eight hours.

In both experiments the lower ground portion heated very decidedly more than the upper part, the tendency to heat being abnormally high in the case of the pebble residue; a thermometer inserted into the ground mass indicated a temperature of 315° C. After half an hour's exposure the deposit cooled very rapidly.

The products obtained in these two experiments were submitted to partial examination, the chief object being to see to what extent the proportions of hyposulphite and sulphide varied in the upper and lower portions of the residue, and the extent to which they were affected by the great difference in the mode of treatment sustained by the different portions of one and the same residue.

The numerical results obtained show that, in both experiments, those portions of the residue which were exposed to the air only for a few seconds, and of which but small surfaces *were* thus exposed (as they were collected in large lumps) contained hyposulphite ranging in amount from 5 to 8·5 per cent. Those portions which were specially treated for the purpose of favouring to the utmost the formation of hyposulphite from sulphide through atmospheric agency, contained, as was to be expected, very large proportions of the former, while the latter had entirely disappeared in three out of the four portions of very finely pulverised residue. In the fourth, however, even after its free exposure to air for forty-eight hours, there still remained nearly 3 per cent. of sulphide. Now, as in no single instance in the entire series of the experiments did any accidental circumstances occur, which even distantly approached the special conditions favourable to the oxidation of the sulphide which were introduced into these particular experiments, the authors consider themselves justified in arriving at the conclusion, that the total absence of sulphide in the residues furnished by the fine-grain powder

in experiments 40, 42, and 47, was not due to accident in the manipulations, and that, in those residues in the general series of analyses which were found to contain large quantities of hyposulphite (as in six out of the nineteen experiments with pebble and R.L.G. powder, and eight out of the nine with F.G. powder) the greater proportion at any rate, of that hyposulphite existed in those particular residues before their removal from the explosion-vessel.

In experiments which the authors have instituted with a sample of sporting powder and one of mining powder, the following course of proceeding was adopted for the removal of the solid residue from the explosion-vessels, and its preparation for analysis. Distilled water, which had been freed from air by long-continued boiling, was syphoned into the explosion-vessel when the latter had cooled, so that air was never allowed to come into contact with the solid residue. When the cylinder was thus quite filled with water it was closed, and set aside for sufficient time to allow the residue to dissolve completely. The solution was then decanted into bottles freed from oxygen, which were quite filled with the liquid and carefully sealed up until required for analysis.

The residues of the two experiments in which these special precautions were adopted to guard against the possible accidental formation of hyposulphite were found to contain respectively 4 and 6 per cent. of that substance. In the series of experiments with pebble powder there were three, in that with R.L.G. powder four, while in that with F.G. powder there was one, of which the residues contained proportions closely similar to those furnished by these two special experiments, there being no peculiarity in those seven experiments nor any attendant circumstances which could be assigned as a possible reason why the proportions of hyposulphite in those cases should be so much lower than in the other experiments with the same powders carried out under the same conditions. The authors, therefore, cannot but conclude that the production of a small or of a larger proportion of hyposulphite (whether as a primary or secondary product, *but before the explosion-vessel is opened*) is determined by some slight modification of the conditions attending the explosion itself.

The sporting powder (Curtis and Harvey, No. 6) used by the authors for the purpose of comparing the results of explosion with those of the military powders used in these researches, did not differ very widely in composition from these; the composition of the charcoal which it contained was intermediate between those contained in the R.F.G. and F.G. powders. No striking points of difference were exhibited between the proportions of the gaseous and solid products which it furnished and those obtained with the four Service powders used. With the mining powder employed the case was very different. This powder presented very important differences in composition from

any of the other powders experimented with, as well as from those used by the recent experimenters referred to in the first memoir. The proportion of saltpetre was about 11 per cent. lower than in the military powders, while the proportions of charcoal and sulphur were higher by about one-half. The composition of the former was similar to that of the pebble powder charcoal. The carbonic oxide produced from this powder was double the highest amount furnished by any of the other powders, while the carbonic anhydride, which in the three series of experiments ranged from 45 to 53 per cent., amounted only to 32 per cent., the two gases existing in about equal proportions. Marsh-gas and hydrogen were present in unusually high proportions, and the sulphuretted hydrogen amounted to 7 per cent., being nearly double the highest proportion found in all the other experiments. The solid residue presented very interesting points of difference. The potassium carbonate was, as might have been anticipated, comparatively small in amount (though some of the experiments with F.G. powder gave similar results in this respect), but there was only 0·5 per cent. of sulphate formed, while the monosulphide amounted to 33 per cent. Federow's experiments are the only ones in which so high a percentage of sulphide is recorded; and among the several experiments with R.L.G. powder, in which only small proportions of sulphate were formed, there was only one residue in which the free sulphur was as high in amount as that formed in the mining powder-residue. The hyposulphite amounted to nearly 6 per cent.—2 per cent. more than was furnished by the sporting powder under *precisely* similar conditions of experiment, and *double* the smallest amount formed in any of the series of experiments conducted with the very special precautions which were applied in dealing with the residue of the powder under discussion. The ammonium sesqui-carbonate was considerably higher in amount than in any other experiments, and the potassium sulphocyanide amounted to 3 per cent., or about five times the amount found in any other experiment excepting that of Link. Lastly, there was a much more considerable amount of residual charcoal in this experiment than in any other.

The very distinctive peculiarities shown by the composition of the solid and gaseous products of this powder are generally such as would have been predicted from the comparatively small proportion borne by the oxidising agent to the oxidisable constituents in the mining powder.

The experiments made with mining powder presented other features of great interest in addition to those elicited by the chemical examination of the products of explosion. In concluding their observations on these, the authors point out that fresh confirmation is afforded by this experiment of the fact that hyposulphite must be classed among the invariable and more important products of explosion of gunpowder in closed spaces.

A comparative examination of the highest, lowest, and mean proportions of solid products furnished by the four military powders with which complete series of experiments (with charges ranging in gravimetric density from 0·1 to 0·9) were made, elicits various points of interest, for which reference must be made to the complete memoir.

Before quitting this portion of the subject the authors refer to a statement appended to their memoir, in which they have given the results of the following calculations:—

(1.) The amount of gaseous products, calculated from the data furnished by the analysis of the solid products.

(2.) The amount of solid products, calculated from the data furnished by the analysis of the gaseous products.

(3.) A comparison between the weights of the elementary substances found in the products of combustion and the weights of the same elements found in the powder prior to ignition.

(4.) The weight of oxygen contained in the total quantity of hyposulphite found.

An examination of this statement will show how closely accordant the various analyses, as a whole, are.

A review of the comparison between the weight of oxygen originally in the powder and that found in the products after explosion, appears to show that there is in the latter, on the average, an appreciable excess of oxygen. Hence it may pretty fairly be concluded that a portion of the hyposulphite found is due to the oxidation of the monosulphide after removal from the explosion-vessel.

On the other hand, a reference to those analyses in which hyposulphite exists in *large* proportions shows that, were the whole of the hyposulphite assumed to be formed after the removal of the products from the cylinders, there would exist a large deficiency in oxygen, and very much larger than the existing excess. Hence it may be equally fairly concluded from this line of argument that it is impossible to attribute to accidental causes the formation of the whole of the hyposulphite, and that a large proportion of it *must* be looked on either as a primary or secondary product.

In passing to a consideration of the heat generated by the explosion of gunpowder, the authors point out that, in their first memoir, they themselves called attention to the inconvenience and to the errors likely to arise from the employment of the particular form of apparatus used, and that they were at the outset fully alive to the great advantages, in regard to saving of time and labour, as well as in respect of accuracy, which would result from the use of an apparatus of the kind since employed by them. But, as one of the chief objects of their experiments was to ascertain whether the results of the metamorphosis of gunpowder, when fired in considerable quantities and under tensions similar to those developed in the bores of guns, were at all similar to

those occurring when small quantities are fired under feeble tensions, the original explosion-apparatus was designedly adopted in the earlier stages of these experiments, so as to determine the heat generated when gunpowder is fired under high tensions, for which purpose vessels of great strength and weight, and therefore not well suited for calorimetric observations, were indispensable.

A further investigation of this branch of the subject has shown, however, that the difference between the authors' earlier determinations of heat and those of other experimenters referred to by MM. Morin and Berthelot are due, not to errors in the former, but to essential differences in the decomposition of different descriptions of powder.

In continuing their heat experiments the authors made use of two explosion-vessels, similar in general form to the larger vessel described in the first memoir, but of small capacity and weight.* The specific heats of both these vessels were carefully determined, and the amount of heat absorbed by the calorimeter for various changes of temperature was also carefully determined. Thermometers specially made for the purpose, and capable of being read to 0°·01 F. (·006 C.), were used in these experiments.

To determine the heat generated, a charge of from 150 to 200 grs. (9·72 to 12·96 grms.) in the smaller cylinder, of 400 grs. (25·92 grms.) in the larger cylinder was carefully weighed and placed in the explosion-vessel. The explosion-vessel was then immersed in the water of the calorimeter and the charge fired in the usual way, the attached thermometer being read before the explosion and afterwards, continuously, until the maximum temperature (which was reached in from two to three minutes) was attained.

To make the new calorimetric determinations as complete as possible, and with the view of exhibiting the differences in the heat evolved due to differences in the composition of the powder, determinations have been made of the heat given off by the three principal powders described in the first memoir, and of three other powders differing widely in their composition, and which have been made the subject of analyses and experiments in these researches, viz.:—Ordinary English mining, Curtis and Harvey's sporting powder, No. 6, and Spanish spherical powder.

With the small explosion-vessel the mean results obtained (Experiments 146 to 166) were as follows, the numbers given below indicating the gramme-units evolved by the combustion of 1 grm. of each description of powder employed.

* One of these weighed 1,381 grms., having a capacity of 32·5 grms.; the other weighed 3,430 grms., and had a capacity of 118·83 grms.

1 grm. of pebble	powder evolved	712·6	units.
1 „ „ R.L.G.	„ „	717·0	„
1 „ „ F.G.	„ „	725·7	„
1 „ „ C. & H. No. 6	„ „	754·3	„
1 „ „ Mining	„ „	508·7	„
1 „ „ Spanish	„ „	762·4	„

With the large explosion-vessel the results, Experiments 171 to 179 and 181 to 192, gave—

1 grm. of pebble	powder evolved	715·5	units.
1 „ „ R.L.G.	„ „	718·6	„
1 „ „ F.G.	„ „	728·0	„
1 „ „ C. & H. No. 6	„ „	756·1	„
1 „ „ Mining	„ „	508·9	„
1 „ „ Spanish	„ „	762·3	„

From the whole of these experiments, and giving to the second series, as probably the more accurate, twice the weight of the first series, we arrive at the conclusion that the heat generated by the combustion of the powders *as actually used* is as follows:—

1 grm. of pebble	powder generates	714·5	gramme-units.
1 „ „ R.L.G	„ „	718·1	„
1 „ „ F.G.	„ „	727·2	„
1 „ „ C. & H. No. 6	„ „	755·5	„
1 „ „ Mining	„ „	508·8	„
1 „ „ Spanish	„ „	762·3	„

From an examination of the whole of the results it is obvious: firstly, that the heat generated by the combustion of gunpowder is subject to very wide variations, dependent upon the particular nature of the powder employed (the Spanish powder for example generates just 50 per cent. more heat than the mining powder); and, secondly, that the heat evolved by the same description of powder varies in different experiments to a greater extent than is to be accounted for by errors of observation. And this was indeed to be expected, since the very considerable variations in the products of combustion under the same circumstances, as disclosed by the analyses, would hardly be supposed to exist without some corresponding variation in the heat evolved.

The authors' views on this head are confirmed by calorimetric determinations in their researches on gun-cotton. In these determinations, which have been carried on with precisely the same apparatus, no appreciable difference was found in the heat evolved in the various experiments.

The units of heat liberated, given above, are those furnished by the

powders as actually used; but as these powders had different amounts of moisture in their composition, and as, in use, these amounts of moisture are found to vary considerably, giving rise, especially when the powders are used in guns, to very different pressures and generating very different energies, it has been considered desirable to correct the above figures, the following being those that would have had place had the powders, when fired, been perfectly free from moisture.

1 grm. dry	pebble	powder	generates	721·4	gramme-units.
1 „ „	W.A.R.L.G.	„	„	725·7	„
1 „ „	W.A.F.G.	„	„	738·3	„
1 „ „	C. & H. No. 6	„	„	764·4	„
1 „ „	Mining	„	„	516·8	„
1 „ „	Spanish pellet	„	„	767·3	„

In their first memoir the authors gave reasons for considering fallacious a temperature of explosion deduced (as has been done by some authors) by dividing the number of gramme-units of heat by the mean specific heat of the exploded powder at 0° C., but for purposes of comparison they give below the temperature of each powder calculated upon the above hypothesis.

Temperature of explosion of	W.A. pebble	powder	3899°	C.
„ „	W.A.R.L.G.	„	3880°	„
„ „	W.A.F.G.	„	3897°	„
„ „	C. & H. No. 6	„	4083°	„
„ „	Mining	„	2896°	„
„ „	Spanish pellet	„	4087°	„

The volume of the permanent gases generated by the explosion of each of the six powders is as follows (calculated for 700° C. and 760 millims. pressure, and corrected for the amounts of moisture they contained):—

1 gramme	W.A. pebble	powder	generates	278·3	cub. centims.
1 „	W.A.R.L.G.	„	„	274·2	„
1 „	W.A.F.G.	„	„	263·1	„
1 „	C. & H. No. 6	„	„	241·0	„
1 „	Mining	„	„	360·3	„
1 „	Spanish pellet	„	„	234·2	„

It is of high importance to observe that the volume of the permanent gases generated is in every case in inverse ratio to the units of heat evolved, as is shown by the following arrangement of the results already given:—

Nature of powder.	Units of heat per gramme exploded.	Cub. centims. of gas per gramme exploded.
Spanish pellet powder	767·3	234·2
Curtis and Harvey's No. 6.....	764·4	241·0
W.A.F.G.	738·3	263·1
W.A.R.L.G.	725·7	274·2
W.A. Pebble	721·4	278·3
Mining	516·8	360·3

The results given in this table are very striking. Taking the two natures of powder which commence and close the list, the heat generated by the Spanish powder is about 50 per cent. higher than that generated by the mining powder, while the quantity of permanent gases evolved by the latter is about 50 per cent. greater than that given off by the former.

Thus it appears that the great inferiority of heat developed by the mining powder, as compared with the Spanish powder, is compensated, or at least approximately so, by the great superiority in volume of permanent gases produced. A similar relation is observed in respect to the other powders, and it would indeed appear that the pressures at any given density and the capacity for performing work of the various powders are not very materially different.

This fact has been entirely verified for the whole of the Waltham Abbey powders, and in a less degree for the three other powders also.

The peculiarities shown by the mining powder are so interesting that it appeared important to determine its tension when fired under a high gravimetric density. 11,560 grs. (749 grms.) of this powder have, therefore, been fired under a gravimetric density of unity. The pressure developed by two very accordant observations was, when corrected, 44 tons on the square inch (6,706 atmospheres). The pressure obtained under similar circumstances from Waltham Abbey powder was 43 tons on the square inch (6,554 atmospheres).

It will afterwards be seen that the capacity for performing work of the various descriptions of powder was also found to be not very different, a similarity of result the more remarkable when it is remembered that with, at all events, three of the powders there were striking differences both in their composition and in the decomposition they experience, and when in consequence material variations both in pressures at different densities and in potential energy might have been expected.

With respect to the great difference in heat evolved by the Spanish and mining powders, it appears difficult to resist the conclusion that the small number of units of heat evolved by the latter is in great measure due to the quantity of heat absorbed in placing the very much

larger proportion of the products of combustion in the form of permanent gases. This suggestion would also appear fully to explain the fact alluded to in the authors' first paper, and to which they had been led purely by experiment, namely, "that the variations observed in the decomposition of gunpowder do not, even when very considerable, materially affect either its tension or capacity for performing work."

A comparison between different gunpowders, or a comparison between gunpowder and other explosive agents cannot, therefore, as has been proposed, be determined by a simple measurement of the corresponding units of heat they evolve.

Did such a law hold, the Spanish powder should have more than 50 per cent. advantage over the mining powder, but, as a matter of fact, although not very widely different, the mining powder had the advantage both in respect to the tension observed in a close vessel and to the energy developed in the bore of a gun.

As regards the *actual* temperature of explosion, the results of the further experiments detailed in this paper leave little doubt that the temperature named in the authors' first memoir, viz., 2,200° C., is not far removed from the truth for the principal powders with which they then experimented.

The slight differences or accidents which appear to give rise to not inconsiderable variations in the products of decomposition of gunpowder are obviously nearly sure to give rise to corresponding variations in the temperature of explosion, and therefore this temperature, even in one and the same powder, cannot be supposed to be always identical.

The authors then discuss the constants in the equation expressing the relation between the tension of the products of explosion and the volume these products occupy, as stated by them in their first memoir, and give values of those constants, corrected from the analyses and experiments made since the publication of that memoir, concluding their remarks upon this part of their subject with a table which gives in terms of the mean density of the powder products the tensions which would exist in the bores of guns were perfectly dry powder of normal composition suffered to expand, with or without production of work. The tensions are expressed in kilogrammes per square centimetre, tons per square inch, and atmospheres.

The authors call attention to the great utility of a table (XI) showing the theoretic work which a charge of gunpowder is capable of effecting in expanding to any value, v. The table given by them exhibits the theoretic work for all necessary volumes of v, from $v=1$ to $v=50$, and several illustrations of the use of the table are annexed.

They continue: if it be desired to know the maximum work of a given charge fired in a gun with such capacity of bore that the charge

suffered five expansions during the motion of the projectile in the gun, the gravimetric density of the charge being unity, the table shows that for every pound or kilogramme in the charge an energy of 91·4 foot-tons, or 62,400 kilogrammetres will, as a maximum, be generated.

If the factor of effect for the powder be known, the above values, multiplied by that factor, will give the energy per pound or kilogramme that may be expected to be realised in the projectile.

But it rarely happens, especially with the very large charges used in the most recent guns, that gravimetric densities so high as unity are employed; in such cases from the total realisable energy must be deducted the energy which the powder would have generated had it expanded from a density of unity to that actually occupied by the charge.

Thus, in the instance above given, if we suppose the charge, instead of a gravimetric density of unity, to have a gravimetric density of ·8, which corresponds to a volume of expansion of 1·25, we see from Table XI that from the 91·4 foot-tons, or 62,400 kilogrammetres, above given, there must be subtracted 19·23 foot-tons, or 13,127·3 kilogrammetres, leaving 72·17 foot-tons, or 49,272·8 kilogrammetres, as the maximum energy realisable under the given conditions per pound, or per kilogramme, of the charge.

As before, these values must be multiplied by the factor of effect to obtain the energy realisable in the projectile.

The authors then discuss the causes which in the bore of a gun affect the energy realised by gunpowder, and point out that this energy varies very much with the powders employed, being in this respect dependent upon circumstances, such as the density of the powder, its size of grain, the amount of moisture, chemical composition, nature of charcoal used, &c., &c., but that the energy may also vary considerably even with the same powder, if the charges be not fired under precisely the same circumstances. For example, especially with slow burning powders, the weight of the shot fired exerts a very material influence upon the factor of effect, and the reason is obvious: the slower the shot moves at first, the earlier in its passage up the bore is the charge entirely consumed, and the higher is the energy realised. The same effect, unless modified by other circumstances, is produced when the charge is increased with the same weight of projectile. In this case the projectile has to traverse a greater length of bore before the same relief due to expansion is attained. The higher pressures which consequently rule react upon the rate of combustion of the powder, and again a somewhat higher energy is obtained.

Both these increased effects, of course, correspond to an increased initial tension of the powder-gases, but, especially with the smaller guns, a very great difference in the realised energy may arise from other causes.

Such causes, affecting the energy realised, are then discussed, and in particular the authors draw attention to the effect of retaining the shot in its seat for a greater or less time, thus giving rise in the former case to a more perfect and earlier combustion of the charge. They cite experiments to show that from this cause alone differences of energy, in guns having a calibre of 12 centims. of 13 to 14 per cent. have been obtained.

They add that in cases where the projectile has been removed for a considerable distance from the charge, that is, when there is a considerable air-space between the charge and the projectile, it has been found that the energy developed in the projectile is materially higher than that due to the expansion of the powder-gases through the space traversed by the projectile, and the cause of this appears clear; when the charge is ignited at one end of the bore, and the ignited products have to travel a considerable distance before striking the projectile, these ignited products possess considerable energy, and a portion of this energy will be communicated to the projectile by direct impact.

The authors then compare the energies realised in a gun by the various classes of powder which form the subject of their memoir, and remark that none of the powders, in realised effect, vary more from that generated by the normal Service Waltham Abbey powders, than do occasional samples of these last; a sufficiently curious result, as already remarked, when the differences in the composition and the great differences in the decomposition of the various powders are taken into account.

It is then pointed out that from the principles laid down it is possible, if the maximum chamber-pressure be known, to fix, very approximately, the position of the shot in the bore when the combustion of the charge may practically be considered to be effected.

Values of the "factor of effect" are then discussed, and the authors conclude the memoir by observing that although the agreement between the results of the long and laborious series of experiments and calculations, which they now bring to a close, have far exceeded the expectations they had formed when they commenced their task, it would yet be idle to suppose that many of their deductions, referring as they do to temperatures and tensions far above the range of ordinary research, will not require some subsequent correction.

But, although certain minor points may, as the authors have said, require considerable correction, they have little doubt that the main theories upon which they insist, confirmed as they are by experiments made, or facts obtained, under very great variety of circumstances, may be accepted as, at any rate, close approximations to the truth. It is satisfactory to find that the laws which rule the tensions and temperatures of gases under ordinary circumstances do not lose their

physical significance, but are still approximately applicable, at the high temperatures and pressures they have been considering.

At all events, whether they are right or wrong in taking this view, it appears to them certain that the rules and tables they have laid down, as based on their analyses, experiments, and calculations, may for all practical purposes be accepted as correct, and may, bearing in mind the restrictions to which they have referred in this memoir, be applied to nearly every question of Internal Ballistics.

(Received June 4, 1879.)

Since the authors completed their memoir, a charge of 10lb. of P powder has been fired in 50 per cent. space. The solid products were collected in the form of an exceedingly hard mass, weighing about 3¾lb., the fracture of which exhibited the want of homogeneity frequently alluded to by the authors. On the surface there was a deposit of varying thickness, and of a pale yellow colour, consisting chiefly of small crystals. The chemical examination of a portion of this deposit, rapidly collected, showed it to contain a considerable proportion of potassium hyposulphite. This product was exhibited to the Royal Society when the paper was read.

V. "Note on the Spectrum of Sodium." By J. Norman Lockyer, F.R.S. Received May 28, 1879.

I have lately been engaged in studying the spectrum of Na under new experimental conditions.

In anticipation of a detailed communication, I take leave to state that the vapour given off from the metal after slow distillation in a vacuum for some time shows the red and green lines without any trace whatever of the yellow one.

Hydrogen is given off in large quantities, and at times the C line and the red "structure" are seen alone.

After this treatment the metal, even when red hot, volatilises with great difficulty.

The Society adjourned over the Whitsuntide Recess to Thursday, June 12, the day appointed for the election of Fellows.

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June 12, 1879.

The Annual Meeting for the election of Fellows was held this day.

THE PRESIDENT in the Chair.

The Statutes relating to the election of Fellows having been read, Dr. T. Graham Balfour and Mr. W. C. Roberts were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following candidates were declared duly elected into the Society:—

Prof. John Anderson, M.D.
Rev. Miles Joseph Berkeley, F.L.S.
Sir Henry Bessemer.
Prof. Alexander Crum Brown, M.D.
Walter Lawry Buller, Sc.D., F.L.S.
George Howard Darwin, M.A.
Prof. Joseph D. Everett, M.A., D.C.L.
Prof. Francis Stephen Bennet François de Chaumont, M.D.
Prof. George Downing Liveing, M.A.
George Matthey, F.C.S.
George John Romanes, M.A., F.L.S.
Arthur Schuster, Ph.D., F.R.A.S.
Prof. Harry Govier Seeley, F.L.S.
Benjamin Williamson, M.A.
Thomas Wright, M.D., F.G.S.

Thanks were given to the Scrutators.

June 19, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Rev. Miles Joseph Berkeley, Sir Henry Bessemer, Prof. Alexander Crum Brown, Mr. George Howard Darwin, Prof. Francis Stephen Bennet François de Chaumont, Prof. George Downing Liveing, Mr. George John Romanes, Dr. Arthur Schuster, Prof. Harry Govier Seeley, Mr. Benjamin Williamson, and Dr. Thomas Wright were admitted into the Society.

The following Papers were read:—

I. "On the Existence of Liebreich's Protagon in the Brain." By Professor ARTHUR GAMGEE, M.D., F.R.S., Owens College, Manchester, and ERNST BLANKENHORN, Ph.D. Received April 29, 1879.

In the year 1865, Dr. Oscar Liebreich published an important paper* upon a new proximate principle which he had separated from the brain. Unlike the numerous bodies, possessed of ill-defined properties which had, by different writers, received the names of cerebrin, cerebric acid, lecithine, or phosphorized fats, this new body could be extracted by an easy process in a state of purity, and to it, probably as indicating it as the first definitively ascertained specific constituent of brain, Liebreich ascribed the name of Protagon (*πρωταγος*, leading in advance).

The substance was obtained by the following process. An animal was bled to death from the carotid, and a stream of water was passed through the vessels of the head, so as to wash the blood out. The brain, freed from its membranes, was then pounded in a mortar and shaken in a flask with ether and water at 0° C. It was allowed to stand at a temperature of 0°, until the ether had separated, and the treatment with ether again and again repeated.

The brain matter having been separated by filtration from ether and water, was digested with 85 per cent. alcohol, at a temperature of 45° C. The fluid was filtered hot and allowed to cool at 0° C. A flocculent precipitate then separated, which was collected on a filter and treated with cold ether, until it ceased to dissolve cholesterin. The insoluble mass was dried *in vacuo*, and dissolved in spirit at 45° C. The alcoholic solution was filtered and allowed to cool *very gradually*, when protagon separated in the form of microscopical needles, differing a little in arrangement and form according to the concentration of the solution. The substance thus obtained could be recrystallized repeatedly. As a result of his analyses Liebreich ascribed to protagon the formula $C_{116}H_{241}NO_{22}P$.

The following exhibits the percentage composition of protagon according to this formula and the mean of all Liebreich's analyses :—

	Calculated for $C_{116}H_{241}N_4O_{22}P$.	Found.
C	67·21	66·74
H	11·59	11·74
N	2·70	2·80
P	1·50	1·23
O	17·00	17·49

* Oscar Liebreich, "Ueber die Chemische Beschaffenheit der Gehirnsubstanz," "Annalen der Chemie und Pharmacie," Bd. cxxxiv, s. 29–44.

Protagon is soluble with difficulty in cold, but more easily in warm alcohol and ether. At higher temperatures than 55° C., alcohol appears to decompose protagon. In water protagon swells and presents the appearance of an opaque jelly, ultimately dissolving so as to form an opaque solution. Liebreich found that protagon was soluble in glacial acetic acid, which deposited it again in a crystalline form, when subjected to the action of cold.

When boiled with a solution of barium hydrate protagon is decomposed, yielding glycerin—phosphoric acid, fatty acids of which he isolated stearic acid in a state of purity, and a base to which he gave the name of *neurine*, and to the platinum compound of which he ascribed the formula $C_5H_{14}NCl_3Pt$. This base was afterwards shown to be identical with the base which Strecker had separated from bile and termed *choline*.

Although the absolute accuracy of a large number of Liebreich's facts has been placed beyond question, the cardinal fact itself—that protagon is a definite phosphorized principle contained in nervous matter—has come to be universally denied.

By Diaconow, Hoppe-Seyler, and Thudichum it is denied that any such definite substance exists, and Liebreich's protagon is held to be a mechanical admixture of a phosphorized body termed *lecithin*, $C_{44}H_{90}NPO_9$, with a nitrogenous, non-phosphorized, body termed *cerebrin*. The presence of phosphorus in protagon is said to be due to contamination with lecithin, and in support of this view it is alleged that by extracting protagon with ether, the substance loses more and more phosphorus; according to Diaconow and Hoppe-Seyler's admission, the phosphorus does adhere most obstinately and cannot be entirely got rid of, though Dr. Thudichum thinks he has, by mere extraction with ether, obtained cerebrin (or cerebrines) quite free from phosphorus.

It is not our intention in this communication to enter into a full criticism of the views of those who have preceded us in the investigation of the existence or non-existence of protagon, and we shall confine ourselves to a statement of a few of the chief objections which appeared to us as insurmountable obstacles in the way of accepting Diaconow's and Hoppe-Seyler's statements.

Liebreich's protagon was described by its author as a white non-hygroscopic substance, which could, by suitable treatment, be constantly obtained in a crystalline form, and this description will be shown in the sequel to be absolutely correct.

Cerebrin is a body most imperfectly known, obtained by its discoverer, Müller,* by pounding up brains with baryta water to the con-

* Dr. Wilhelm Müller. "Ueber die Chemischen Bestandtheile des Gehirns-zweite Abhandlung." "Annalen der Chemie und Pharmacie." Bd. cv (1858), s. 361.

sistence of a thin milk and then boiling; a precipitate forms, which is extracted with boiling alcohol. On cooling, the fluid deposits an abundant precipitate; this is treated with ether, to separate cholesterin and fats, and then recrystallized from boiling alcohol. Of this body Müller published two analyses, which led him to assign to it the formula $C_{34}H_{33}NO_6$. It is described by Hoppe-Seyler as a light, white, *highly hygroscopic* powder.*

But what right have we to assume that a body obtained by boiling so complex an organic mixture as a brain with caustic baryta is a proximate principle of the brain, *i.e.*, that it exists preformed in the brain? The surmise is as bold as it is improbable.

Lecithin is described by Diaconow himself as a non-pulverizable, yellowish-white, *highly hygroscopic* body.† Is it not strange that two usually amorphous highly hygroscopic bodies should, when mixed, give rise to a constantly crystallizable absolutely non-hygroscopic mixture? And this is exactly what would be the case were the hypotheses of Diaconow and Hoppe-Seyler correct.

According to Diaconow, by repeated extractions with ether, the P contained in protagon may be made to sink to 1 per cent., whereas, according to Liebreich's formula, it should contain 1·5 per cent. Actually, in the three determinations which he made, Liebreich obtained 1·1, 1·1, and 1·5, as the amount of phosphorus in 100 parts, but, unfortunately, he seems to have concluded that the highest number was correct and made it the basis of his calculation.

In the year 1877, one of us, assisted by Mr. Leopold Larmuth, Platt Physiological Scholar, commenced in the Physiological Laboratory of Owens College, a series of experiments intended to determine whether Liebreich's protagon existed or not. This preliminary investigation showed that by Liebreich's process there is always obtained a body having the physical properties of protagon, and containing phosphorus in a proportion sufficiently near to that indicated by him; it was found that the amount of phosphorus in specimens of protagon which had been crystallized from alcohol four or five times, was not smaller than that present in protagon which had only once been crystallized, though a thorough treatment with ether preceded each recrystallization.

These first experiments so far as they went, were perfectly satisfactory. It appeared, however, quite essential, before forming a definite opinion, to extend them very considerably and especially to

* "Auf diese Weise dargestellt bildet das Cerebrin ein leichtes weisses sehr hygroscopisches Pulver," &c. Hoppe-Seyler, "Handbuch der Physiologisch—und Pathologisch—Chemischen Analyse." Dritte Auft. S. 169.

† "Das reine Lecithin stellt eine gelblich-weisse, wachsartige, in dünner Schicht-ausgebreitet seiden-glänzende, sehr hygroscopische Masse dar." Diaconow, "Ueber die Chemische Constitution des Lecithin." "Centralblatt f. d. Med. Wissenschaften," 1868, p. 2.

prove the definite nature of protagon by a large number of analyses, indicating not merely the amount of phosphorus, but also that of the other elements present in it.

This investigation we have lately been able to carry out with all necessary minuteness, and having, to our entire satisfaction, established the real existence of protagon, we communicate the results obtained before proceeding to a fresh study of the products of decomposition of that body.

Process employed in the preparation of Protagon in the present research.

At first we commenced by repeating exactly the process of Liebreich in all its details; one of the steps of that process we had found fraught with peculiar difficulty, and we soon ascertained that it could be dispensed with without prejudicially affecting the success of the operations. The step to which we refer consists in agitating the freshly pounded brain repeatedly with water and ether at 0° C., so long as the ether dissolves considerable quantities of substance, then filtering and placing the insoluble matter into 85 per cent. alcohol at 45° C. When pounded brain is so treated with water and ether it swells up and the separation of the ether is most incomplete. The process of filtration is one which is attended with great difficulty, even when carried out in the only way in which we found it possible, viz., in the woven bags sold for household purposes for straining jellies, &c. By the kindness of Professor Osborne Reynolds, F.R.S., a small model of a centrifugal hydro-extractor, similar to that used in sugar refineries, was constructed for us in the engineering workshop of the Owens College, and was driven by the laboratory steam-engine, and this was for some time efficiently employed in separating the portion of the brain insoluble in water and ether from these fluids. It was, however, obvious that however prolonged the ether washing, it never succeeded in freeing the brain from cholesterin and other matters soluble in ether, and that the removal of these bodies from protagon was most readily effected at a later stage of the operations. As, ultimately modified, our process was as follows:—

Perfectly fresh ox's brains were freed from blood and from adhering membranes as completely as possible, and were then digested for many hours (18 to 24) in 85 per cent. alcohol in a large incubator kept constantly at 45° C. The fluid was filtered whilst hot, and the insoluble matter was again treated with fresh quantities of spirit, the proceeding being repeated four or five times, as long, indeed, as the filtrate when cooled to 0° deposited a fair quantity of white flocculent precipitate. This precipitate was collected on a filter, and being then transferred to a stoppered bottle was thoroughly and repeatedly agitated with ether, in order to dissolve cholesterin and other bodies soluble in ether. The ether having been removed, first by decantation

and then by filtration, the substance left undissolved by it was first of all dried between sheets of filtering paper in air, and afterwards over sulphuric acid or phosphorus pentoxide. The resulting mass, which had a snow-white colour, was reduced to powder, moistened with a little water, and digested for many hours with alcohol heated to 45° C. From the filtered liquid, if this be allowed to cool *very gradually*, the protagon separates in the form of microscopic crystals, mostly arranged in rosettes, the appearance and arrangement of which differs somewhat, as Liebreich very exactly pointed out, according to the degree of concentration of the solution. The once crystallized protagon thus obtained was collected on a filter, washed with ether, and dried first of all in air and ultimately over P_2O_5. It was then recrystallized as many times as required, the process always commencing by pulverizing and thoroughly shaking with cold ether.

It appeared to us of special importance not merely to subject to ultimate analysis several different samples of protagon, but specially to subject to ultimate analysis protagon after the second crystallization, and the same protagon after being recrystallized a third time.

In our analyses the C and H were determined by combustions made with lead chromate, with an abundance of reduced copper in the fore part of the combustion tube. The N determinations were made by Dumas' method, the phosphorus was determined in some cases by Carius's method, in others by fusion with sodium carbonate and saltpetre. The results of every combustion, save two, in which unavoidable accidents occurred, are given below.

The following are the results of the analyses:—

1. *Protagon twice recrystallized from ox brain.*

0·288 grm. gave 0·702 CO_2 and 0·284 H_2O.
0·355 „ 7 cub. centims. of N (at 0° C. and 760 millims.).
0·296 (treated by Carius's method) gave 0·0116 grm. of $Mg_2P_2O_7$.

2. *Twice recrystallized protagon from ox brain* (same sample as 1).

0·306 grm. gave 0·747 CO_2 and 0·2955 H_2O.
0·262 „ 6 cub. centims. of N (at 0° C. and 760 millims.).
0·3025 (treated by Carius's method) gave 0·012 grm. of $Mg_2P_2O_7$.

3. *Twice crystallized protagon from ox brain* (different sample from 1 and 2).

0·284 grm. gave 0·691 grm. of CO_2 and 0·270 grm. of H_2O.
0·388 „ 8 cub. centims. of N at 0° C. and 760 millims.
0·782 „ 0·0291 grm. of $Mg_2P_2O_7$.

4. *Twice crystallized protagon from ox brain* (same sample as 3).

0·2968 grm. gave 0·722 CO_2 and 0·288 grm. of H_2O.
0·5368 „ 0·02 grm. of $Mg_2P_2O_7$.

5. *Once crystallized protagon from dog's brain.*

0·305 grm. gave 0·7415 grm. of CO_2 and 0·289 grm. of H_2O. Neither P nor N determined.

6. *Once crystallized protagon from dog's brain* (same sample as 5).

0·25 grm. gave 0·6103 grm. of CO_2 and 0·249 grm. H_2O.

7. *Four times crystallized protagon* (prepared in 1877 by Mr. Larmuth).

0·214 grm. gave 0·5201 grm. CO_2 and 0·202 grm. H_2O.

8. *Three times crystallized protagon* (prepared by recrystallizing the body which had furnished the samples used in Analyses 3 and 4).

0·214 grm. gave 0·5203 grm. CO_2 and 0·2015 grm. of H_2O.
0·48 „ 9·7 cub. centims. of N (at 0° C. and 760 millims.).
0·5164 „ 0·019 grm. of $Mg_2P_2O_7$.

The results are shown in a tabular form below:—

	Protagon once recrystallized (dog).		Protagon twice recrystallized (ox).		Protagon twice recrystallized (ox).		Thrice recrystallized (ox).	Four times recrystallized (horse).
	No. 5.	No. 6.	No. 1.	No. 2.	No. 3.	No. 4.	No. 8.	No. 7.
C.	66·3	66·6	66·46	66·58	66·34	66·35	66·30	66·26
H.	10·52	11·06	10·96	10·72	10·56	10·78	10·467	10·48
N.	..	..	2·3	2·6	2·40	..	2·29	..
P.	..	..	1·094	1·107	1·032	1·081	1·027	..
O.								

From the above numbers we have deduced for protagon the empirical formula $C_{160}H_{308}N_5PO_{35}$.

	Calculated.			Mean found.
C_{160}	1920	.....	66·45	..... 66·39
H_{308}	308	.....	10·66	..... 10·69
N_5	70	.....	2·42	..... 2·39
P	31	.....	1·07	..... 1·068
O_{35}	560	.....	19·40	
	2889		100·00	

A study of our results leaves no doubt of the definite and, within certain limits, stable character of protagon. It is to be borne in mind that in no case were the solutions containing protagon exposed to a higher temperature than 45° C., as we have good reason to believe that Liebreich was right in asserting that at higher temperatures protagon undergoes decomposition.

II. "Note on the Existence of a Milk-curdling Ferment in the Pancreas." By WILLIAM ROBERTS, M.D., F.R.S., Physician to the Manchester Royal Infirmary. Received May 10, 1879.

In the course of some observations on the digestion of milk by extract of pancreas, I found that the milk passed through a more or less pronounced phase of curdling, which often considerably delayed the complete peptonising of the casein. As the property of curdling milk has hitherto been regarded as the special appanage of the gastric ferment, I was unprepared to find it also associated with the ferments of the pancreas. There is, however, no doubt about the fact, at least in regard to the pancreas of the pig, the ox, and the sheep.

It was found that extract of pancreas made with saturated solution of sodium chloride had much stronger curdling powers than the glycerine extract, whereas the latter had stronger proteolytic powers than the former. This indicates that the curdling ferment of the pancreas is distinct from the proteolytic ferment (the trypsin of Kühne), just as it has been recently shown that the curdling ferment of the stomach is distinct from pepsine.

The brine extract of pancreas, or pancreatic rennet, as it may be called, seems to act on milk exactly in the same way as rennet made from the calf's stomach. It coagulates casein actively, both in neutral and in alkaline milk, and it may be assumed as probable—at least until further inquiry—that the curdling agent of the stomach and the curdling agent of the pancreas are one and the same ferment.

III. "On some Recent Improvements made in the Mountings of the Telescopes at Birr Castle." By the EARL OF ROSSE, D.C.L., LL.D., F.R.S.

(Abstract deferred.)

IV. "The Measurement of the Ratio of Lateral Contraction to Longitudinal Extension in a Body under Strain." By A. MALLOCK. Communicated by Lord RAYLEIGH, F.R.S. Received May 17, 1879.

The three coefficients which define the elastic properties of isotropic solids, viz., the simple rigidity (n), Young's modulus (q), and the elasticity of volume (k) are connected by the equations

$$2n\,(\mu+1)=q=3k\,(1-2\mu).$$

The quantity μ or the ratio of lateral contraction to longitudinal extension, the measurement of which forms the subject of this paper,

appears in many problems relating to the flexure of solids, and has besides a kind of historical interest in virtue of Poisson's erroneous conclusion that it must be $\frac{1}{4}$ for all bodies.

Various methods have been employed to measure the ratio. The most obvious perhaps after direct measurement (which is obviously only applicable to a very small class of bodies) is to compare the times of longitudinal and torsional vibration of a wire or cylinder of the substance.

But as by this method $\mu=\frac{V_q^2-2V_n^2}{2V_n^2}$ where V_q and V_n are the velocities of the vibrations which depend on q and n respectively, it is easily seen that large errors may appear in the value deduced for μ when only comparatively small errors are made in the measurement of V_q and V_n, and besides this it is not unlikely that the q and n determined by vibrations, when there is not time for the heat caused by the strain to alter its distribution by conduction, may not be the same as when determined by statical methods. Wertheim made use of tubes closed at one end and filled with water, the surface of which appeared in a capillary tube fixed at the other end. The necessary data were obtained by subjecting the tubes to longitudinal strain, and observing the extension produced by it and the depression of the water in the capillary tube. Kirchhoff has employed an ingenious method in the case of brass and steel rods.

It depended on observing simultaneously the torsion and flexure produced by hanging a weight on an arm attached in a horizontal plane at right angles to one end of the rod, the other end of the rod being fixed in a horizontal position.

His experiments seem to have been made with every precaution against error, and the values he deduces for μ are ·294 for steel and ·387 for brass, both of which are larger than the values found in my experiments on the same metals.

This may be attributed in part, at any rate, to the fact that Kirchhoff used hardened steel and drawn brass rods, whereas most of my specimens were annealed.

The method which I have employed has some advantages over these, especially in this that it is applicable to almost every solid, no matter how brittle it may be, or how narrow are its limits of elasticity.

It depends on the proposition that when a rectangular bar with plane sides is bent by opposing couples whose planes are parallel to one pair of sides, the other pair become surfaces of uniform anti-elastic curvature, with principal radii of curvature R and $\frac{-\mu}{R}$, provided that R is large compared with the third proportional to the thickness and breadth of the bar. (See Thomson and Tait "Nat. Phil.," § 716.)

If R_1 and R_2 are the radii of curvature parallel to the length and

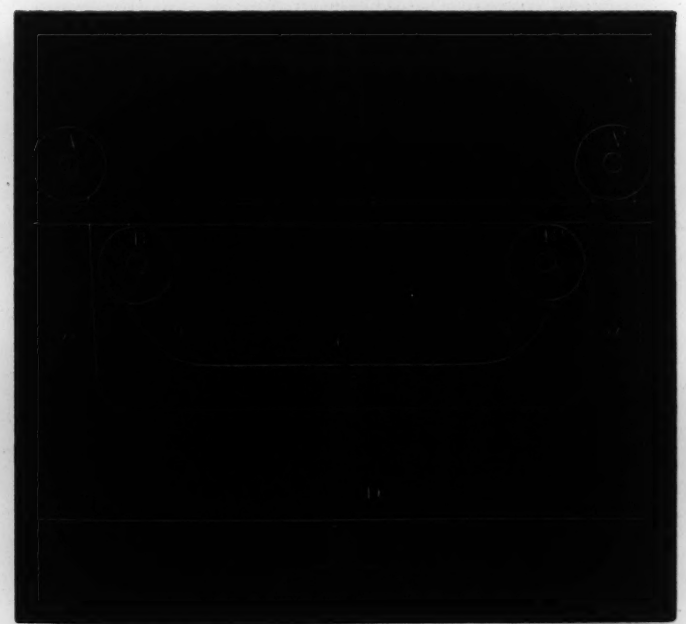
breadth of the bar respectively, $\mu = -\frac{R_1}{R_2}$, I measure this ratio as follows—

Fig. 1.



On one side of a rectangular bar, fig. 1, of the substance for which μ is to be measured, and about the middle of its length, a circle is described of diameter nearly equal to the breadth of the bar. Diameters are drawn in this circle parallel and perpendicular to the length of the bar, and at their extremities fine holes are drilled normal to the surface. (The dimensions of these holes are so small compared to those of the bar, that they do not sensibly affect the flexure.) Four fine steel wires of equal length are planted firmly in these holes, and their free ends are then bent together so that they may all be in the field of a microscope at the same time. The bar is then placed in a frame of which fig. 2 is a section.

Fig. 2.



AA′ BB′ are pairs of rollers between which the bar is held. A and A′ are mounted on the outer frame *aa′*, BB′ on the frame *bb′* which can be advanced towards AA′ by the screw D; this screw has

a spherical head fitting into a spherical cavity in bb' at C, and C is exactly half-way between A and A′, and B and B′. When the bar is bent in this frame by turning the screw D, the bending is accomplished by pure, equal, and opposite couples; for since AA′ BB′ are rollers and free to turn, there can be no tractions along the face of the bar, and therefore the pressures are normal to the surface, and the couples are equal in virtue of the equality of the distances AC A′C and BC B′C. Thus for that part of the bar between B and B′ the radii of curvature R_1 and R_2 are constant, except in so far as they may be affected by the surface distribution of pressure at B and B′ which as may readily be seen by the transverse section fig. 3, would tend

FIG. 3.



to make R_2 too large in the neighbourhood of the rollers; but the effect produced by this cause must be quite insignificant towards the middle of the bar. The whole frame aa' is mounted on a platform with screw motions parallel and perpendicular to the length of the bar, and the free ends of the wires are focussed in a fixed microscope,

FIG. 4.



Let h_1, h_2, k_1, k_2, fig. 4, be the ends of the wires. The distances (h_1h_2) (k_1k_2) resolved parallel and perpendicular to the length of the bar respectively, are measured by the screw motions, first, when the bar is unstrained, and again when it is bent by turning the screw D.

Let h, k, h', k' be these distances, then if

ϵ=length of the wires,

r=radius of circle drawn on bar,

θ=angle made by the wire with the normal to bar,

the increment of the distance between h_1 and h_2 when the bar is bent, or $h'-h$ is

$$\frac{2\epsilon r\cos\theta}{R_1},$$

and the decrement of the distance between k_1 and k_2 or $k-k'$ is

$$\frac{2\epsilon r \cos\theta}{R_2},$$

hence

$$\frac{k-k'}{h'-h}=-\frac{R_1}{R_2}=\mu.$$

The limit to the smallness of curvature which can be employed to measure μ by this method depends on the smallness of the interval which can be satisfactorily measured by the microscope and micrometer screw. In my experiments $h'-h$ varied from ·015 to ·03 inch, ϵ being about 3 inches and r nearly ·5 inch. These values would make R_1 vary from 200 inches upwards. The dimensions of the bars employed were about $8\times1\times\cdot25$ inches, so that the third proportional to the thickness, breadth (viz., 4 inches), fulfils the condition of being small compared to R. In fact, I found that much larger curvatures might be employed without sensibly altering the values obtained for μ.

My method admits of the determination of μ for substances which are not isotropic. Some examples of this are given in the table below in the case of the woods box, beech, and deal; μ_ρ, μ_σ, are the values of μ parallel and perpendicular to the radial plane of the tree, *i.e.*, the plane which contains the medullary fibres; μ_τ is the value in the plane cutting the axis of the tree at right angles.

It will be noticed that in all three cases μ_σ is greater than μ_ρ.

Beech-wood seems even to grow rather less dense in a radial direction under longitudinal pressure, but this is the only instance in which I found a value for μ greater than ·5.

Values of μ for various Substances.

Substance.	μ	μ_σ	μ_ρ	μ_τ
Steel	·253			
Brass	·325			
Copper	·348			
Lead	·375			
Zinc (rolled)	·180			
Ditto (cast)	·230			
Ebonite	·389			
Ivory	·50			
India-rubber	·50			
Paraffin	·50			
Plaster of Paris	·181			
Card-board	·2			
Cork	·00			
Box-wood	..	·42	·406	
Beech-wood	..	·53	·408	
Deal (white Pine)	..	·486	·372	·227

V. "The Motion of Two Spheres in a Fluid." By W. M. Hicks, M.A., St. John's College, Cambridge. Communicated by Professor J. Clerk Maxwell, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received May 16, 1878.

(Abstract.)

In the investigation the method of images is followed, and is based on the following lemma. The "image" of a source in an infinite fluid in presence of a sphere consists of a source at the inverse point of the former, and a line sink thence to the centre of the sphere. If the original source be μ at P, the inside source $=\frac{a}{OP}\mu$, and the line density of the sink $=-\frac{\mu}{a}$, a being the radius of the sphere. For the analogous case of fluid within a sphere we, of course, require to have an equal source and sink somewhere within, else there must ensue motion across the boundary. In this case the analysis would give an infinite term for a single source, which, when the quantity of source and sink is zero, disappears. The image for a source would be as before a source $\frac{a}{OP}\mu$ at the inverse point of P, and a line sink $-\frac{\mu}{a}$, thence to infinity. It is this last which produces the infinite term. From these lemmas are deduced the images of "doublets," by which are meant the singular points resulting from the coalescence of an equal source and sink whose magnitude varies inversely as the distance between them. It is easily seen that when the axis of a doublet passes through the centre of the sphere, its image is a single doublet within it, at the inverse point of the former of magnitude $-\left(\frac{a}{OP}\right)^3\mu$, μ being the strength of the original doublet. A similar result holds for the image of a doublet within a sphere, whose axis passes through the centre of the sphere.

The image of a doublet whose axis is perpendicular to the line to the centre is not so simple. It consists as before of a doublet $\left(\frac{a}{OP}\right)^3\mu$ at the inverse point, but besides this there stretches from the inverse point to the centre a negative line doublet of variable density, the line density at a distance, r, being $-\frac{\mu}{a}\frac{r}{OP}$. The only difference between this and the case for an internal doublet is that the corresponding line doublet stretches to infinity.

No attempt is made to find the velocity potentials, but supposing them known, the kinetic energy of the motion for two spheres is

expressed in terms of the magnitudes of the doublets also supposed known. The kinetic energy of the fluid motion is of the form

$$2T=A_1u_1^2+B_1(v_1^2+w_1^2)+A_2u_2^2+B_2(v_2^2+w_2^2)-2Lu_1u_2+2M(v_1v_2+w_1w_2),$$

where u_1, u_2 denote velocities along the line of centres, and v_1, w_1 .... perpendicular to it, and it is shown that

$$A_1=\frac{1}{2}m_1u_1^2\{1+3\Sigma_1^\infty\mu\},$$

where m_1 is the mass of fluid displaced by the sphere, and $\Sigma_1^\infty\mu$ denotes the sum of all the doublet-masses within (1) due to a unit motion of (1) along the line of centres. A similar formula holds for B_1, the $\Sigma\mu$ in this case denoting the doublet-masses within (1) due to a unit motion of (1) perpendicular to the line of centres. Also that

$$L=\frac{3}{2}m_2\Sigma_1^\infty(\mu')+\frac{3}{2}m_1\Sigma_1^\infty(\nu),$$

where $\Sigma_1^\infty(\mu')$ denotes summation for all the images inside (2) due to a unit motion of (1) along the line of centres, and $\Sigma_1^\infty(\nu)$ summation of all the images in (1) due to a corresponding motion of (2). An analogous expression holds for M. A_1, A_2, L, are completely determined in terms of the radii of the spheres and their distances, and an approximation is made to the values of B_1, B_2, and M. In certain cases the values for the coefficients take up simple forms; for instance, at the moment when one sphere is concentric with the bounding one $A_1=\frac{1}{2}m_1\frac{b^3+2a^3}{b^3-a^3}$, a result already obtained by Stokes.

The motion of the spheres along their line of centres is considered, and it is shown that whatever be their velocities, the effect of the fluid motion is such that relatively they appear to repel one another. More particularly too is considered the case of motion of a single sphere in an infinite fluid bounded by a plane. Amongst other things, it is shown that the ratio of the limiting velocity to the velocity when in contact with the plane $=\sqrt{\left\{\frac{2\rho+1\cdot6061707}{2\rho+1}\right\}}$, ρ being the density of the sphere.

Finally also is considered the effect of the fluid on vibratory motions and the mutual influence of two pendulums when they oscillate in their line of centres; data are given in the paper for the determination of the effect when they move in any manner. More particularly the effect on a sphere is considered when another has impressed on it a small harmonic vibration in the line of centres, and the force required to keep it at rest. In the first case the mean effect is an acceleration towards the vibrating sphere (A_1)

$$=-\frac{v^2}{A_2}\frac{d}{dr}\left(\frac{A_1A_2-B^2}{A_2}\right),$$

where v is the "velocity of mean square" of (A_1).

When the distance of the spheres is so large that we may neglect twelfth and higher inverse powers of the distance this attraction is

$$\frac{18v^2}{2\rho+1}\cdot\left(\frac{a}{r}\right)^7\frac{1}{a}\left\{\left(\frac{r^2}{r^2-b^2}\right)^4-\frac{3}{2\rho+1}\right\},$$

a, b, being the radii of the vibrating and free sphere respectively. From this it may be shown that if the density of the sphere is greater than the fluid it is attracted, whilst if less it will be attracted or repelled, according as its distance is less or greater than a certain critical distance, which is given by $r=\dfrac{b}{\sqrt{\left\{1-\sqrt[4]{\dfrac{2\rho+1}{3}}\right\}}}$. For instance, if $\rho=\cdot9$, this distance $=7\cdot648$, the radius of the free sphere. These formulæ are obtained on the supposition that the spheres are not so close that $\frac{dA_1}{dr}$ are too great to be neglected, for at contact $\frac{dA_1}{dr}$ are infinite. When the sphere is held fast, the mean force required to do so is $-v^2\frac{dA_1}{dr}$, which, when we neglect twelfth powers of $\frac{1}{r}$, $=\frac{v^2}{g}\frac{9a^3b^3r}{(r^2-b^2)^4}\times$ weight of fluid displaced by the vibrating sphere; for example, for two oxygen atoms at 0° C., at a distance four times their radius, the force is about 78×10^6 weight of fluid displaced by one atom, thus while the force decreases indefinitely, the effective force increases indefinitely.

For two spheres ($a=b=\frac{1}{4}$ inch $r=4a$) vibrating through a distance $\frac{1}{10}$th of an inch 256 times a second, in water, the force is equal to the weight of 12·8 milligrams.

VI. "Microscopical Researches in High Power Definition." By G. W. Royston-Pigott, M.A., M.D. Cantab., F.R.S. Received May 23, 1879.

(Abstract.)

In its general scope the paper is intended to deal with difficulties in microscopic research, usually found insuperable—such, for instance, as the invisibility of minute *closely packed* refracting spherules, existing in double rouleaux, or promiscuously aggregated, when their individual diameter varies between the 1-80,000th to the 1-200,000th of an inch.

These difficulties are principally created by overlapping images—due partly to residuary aberrations both spherical and chromatic—partly tc thc effects of diffraction, caused by brilliant illuminations of spurious disks of light—partly to the constant development of Eidola or false images, which vary the loci of their development according to the nature of underlying structures and according to the object-glasses being over or under corrected; and they are partly, and indeed very considerably, created by the use of excessively large angular apertures.

The paper discusses also, the relative effects on visibility, of large and small angular apertures in objectives.

It shows that the black margins or black marginal annuli of refracting spherules, constantly displayed by low aperture glasses, are attenuated gradually to invisibility as the glasses employed are endowed with the largest apertures. That the black margins also of cylinders, tubules, or semi-tubules suffer similar obliterations. And that, in consequence, innumerable minute details are concealed or destroyed till the aperture is sufficiently reduced.

That minute refracting bodies obey the laws of their refrangibilities and display beautiful phenomena, discoverable by transcendent powers of definition; but totally unseen by inferior compensations. And that, in consequence, the so-called achromatism of modern glasses is an illusory approximation to correct vision.

Examples are given of molecular structures, varying in form, translucency, and refrangibility, in which natural pencils are caught and displayed in the order in which, as in a rain drop, iridescent rays are emitted by the decomposed light.

Several examples are also introduced, in which a high order of lenticular correction beautifully discovers structure hidden, according to Dr. Carpenter, F.R.S., from the great bulk of observers.

As the paper deals so often with magnitudes very much less than the 1–100,000th of an inch, a method is introduced of readily estimating roughly such magnitudes between the 1–80,000th and the 1–500,000th of an inch, by means of a micrometer gauge. The writer has been emboldened to grapple with these difficult minutiæ, in consequence of the sharp and clear definition he has attained of spider lines miniatured down to the fourteenth part of a hundred-thousandth of an inch. The eye, accustomed to contemplate this subtlety of form, readily appreciates the one-fourth or sixth of this size, *i.e.*, 1–400,000th or 1–600,000th.

The writer has also ventured to bring before the notice of the Royal Society, a new test for the microscope, displaying bright lines of uniform thickness less than the 1–100,000th, and sharp black lines of much less tenuity than those given by Nobert's celebrated lines ruled on glass, and incomparably more easy of illustration.

The employment of various fluids for immersion lenses is carefully considered; and the singular property of castor oil discovered by the writer is referred to.

As the Society honoured the writer by inserting in their "Transactions" a paper on "A Searcher for Aplanatic Images," he now introduces a new form which offers some advantages: by its extended traverse, by its simplicity and economy of light with increase of magnifying power.

Finally, some examples are given of producing transcendent definition in cases found hopeless by a numerous body of observers; as the papers written in "The Monthly Microscopical Journal" during the last ten years abundantly demonstrate. The means also of its attainment are minutely described.

VII. "Note on 'Spectroscopic Papers.'" By G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received May 29, 1879.

In a recent communication to the Royal Society, Mr. Lockyer has criticised our statement of Young's wave-length identifications of certain chromospheric lines. As to the wave-length, we have throughout our table omitted all figures after the decimal point merely for the sake of not cumbering the table. The numbers, Young tells us, are not his own, but taken from Ångström's catalogue. Moreover, as to Young's identifications with metallic lines, he states expressly that they were taken from the maps of Kirchhoff, Ångström, and Thalén, and Watts's "Index of Spectra." But our object was not to criticise Young's work, but only to use it for the purpose of comparing the behaviour of certain metals on the earth and in the sun, and the conditions under which certain lines appear, or do not appear, or are reversed.

We should perhaps have made our meaning clearer if we had given another column with the wave-lengths of the metallic lines referred to side by side with Young's numbers for the chromospheric lines. We mentioned, in relation to aluminium, the two lines with wave-lengths 6245·4 and 6237·3 seen by Young, not because we thought their identity with the aluminium lines proved, but because they are the only two lines in Young's table which are at all close to aluminium lines, and if they be not due to that metal, then we have the remarkable fact that aluminium in the sun gives no lines either dark or bright except the two which have been reversed on the earth. A somewhat similar remark applies to the potassium lines, only in that case Young's line has a wave-length very nearly the mean of the two

potassium lines which are often seen as one, not merely from want of dispersive power (which with prisms is usually sufficient at the violet end), but because the lines are expanded until they meet. We at first saw them reversed as one broad line which divided in two as the potassium evaporated.

Mr. Lockyer complains that we omitted to refer to his work on the wave-length of the potassium lines. In our first communication on the subject of reversals we named at the outset those who had been previously engaged in the same field, and after referring to their work in general terms we did not think it desirable subsequently to notice the specific results obtained by each individual. As neither the existence of the two potassium lines nor the exact determination of their wave-lengths, but only the identification of the dark lines we observed with the bright lines was in question, we considered it unnecessary to mention specially Mr. Lockyer's work on those lines.

We owe it to the Society to explain the value which is to be attached to the wave-lengths mentioned by us. We have never set ourselves to determine exact wave-lengths; but we sought to determine the conditions of reversal, and we used wave-lengths for the convenience of indicating the lines of which we wrote. In very few cases have we given any determinations of wave-lengths. In general we determined the identity of a dark line with the corresponding bright line by observing that both had the same place on the cross-wires, or pointer, or both gave the same reading of the scale of the spectroscope. We then gave the wave-length of the bright line as determined by some good authority, usually either Thalén or Boisbaudran. When we could not identify the dark lines in that way, we took the readings of known lines with our spectroscope, using, for lack of sunshine, most frequently Boisbaudran's method and wave-lengths, and having drawn a curve, determined the wave-lengths of the dark lines therefrom. It was in this way that we obtained the wave-lengths of the two dark lines we ascribed to potassium. It is, of course, just within the bounds of possibility that the line for which we obtained the wave-length 4045 (it is misprinted 4044 in the table, but not in the text) may have been an iron line reversed, but, as the principal well-known lines of potassium were at the same time seen bright in the spectrum while those of iron were not seen, it is far more probable, almost certain, that the line was really due to potassium, and the last figure of the wave-length wrongly determined. By giving exactly what we observed, we have enabled our readers to judge for themselves what is most probable in this case. To attempt to determine wave-lengths to any greater degree of nicety would, so far as our purpose was concerned, have been labour thrown away. Whenever the wave-length of a line observed by us to be reversed is given differently from that given by

Thalén and that given by Boisbaudran for the bright line to which we assume it to correspond, it was determined in the way above described, and must not be taken to have more importance. By our method of working, the very high dispersive power required for the specific identification of any substance by the determination of an individual wave-length is avoided; as we depend on the greatly diminished likelihood of error when several groups of lines of the same substance are seen to be continually present at the time of one or more reversals. It was the necessity of being able to rapidly sweep the entire spectrum for the above purpose that caused us to limit the dispersion. A large majority of the wave-lengths given by Thalén were obtained by means of the moderate dispersion of one bisulphide of carbon prism, a less dispersion than we have used; and it would be incorrect to suppose that no enduring work in this field of spectroscopy can be effected except with the enormous dispersive power which Mr. Lockyer recommends.

VIII. "The Determination of the Secular Effects of Tidal Friction by a Graphical Method." By G. H. Darwin, M.A., formerly Fellow of Trinity College, Cambridge. Communicated by J. W. L. Glaisher, M.A., F.R.S. Received May 31, 1879.

Suppose an attractive particle or satellite of mass m to be moving in a circular orbit, with an angular velocity Ω, round a planet of mass M, and suppose the planet to be rotating about an axis perpendicular to the plane of the orbit, with an angular velocity n; suppose, also, the mass of the planet to be partially or wholly imperfectly elastic or viscous, or that there are oceans on the surface of the planet; then the attraction of the satellite must produce a relative motion in the parts of the planet, and that motion must be subject to friction, or, in other words, there must be frictional tides of some sort or other. The system must accordingly be losing energy by friction, and its configuration must change in such a way that its whole energy diminishes.

Such a system does not differ much from those of actual planets and satellites, and, therefore, the results deduced in this hypothetical case must agree pretty closely with the actual course of evolution, provided that time enough has been and will be given for such changes.

Let C be the moment of inertia of the planet about its axis of rotation;

r the distance of the satellite from the centre of the planet;

h the resultant moment of momentum of the whole system;

e the whole energy, both kinetic and potential of the system.

It will be supposed that the figure of the planet and the distribution of its internal density are such that the attraction of the satellite causes no couple about any axis perpendicular to that of rotation.

Then the two bodies revolve in circles about their common centre of inertia with an angular velocity Ω, and, therefore, the m. of m. of orbital motion is

$$M\left(\frac{mr}{M+m}\right)^2\Omega+m\left(\frac{Mr}{M+m}\right)^2\Omega=\frac{Mm}{M+m}r^2\Omega.$$

Let μ be attraction between unit masses at unit distance.

Then, by the law of periodic times, in a circular orbit, $\Omega^2r^3=\mu(M+m)$ whence

$$\Omega r^2=\mu^{\frac{1}{3}}(M+m)\Omega^{-\frac{1}{3}}.$$

And the m. of m. of orbital motion $=\mu^{\frac{1}{3}}Mm(M+m)^{-\frac{1}{3}}\Omega^{-\frac{1}{3}}$.

The m. of m. of the planet's rotation is Cn,

and therefore
$$h=C\left\{n+\mu^{\frac{1}{3}}\frac{Mm}{C}(M+m)^{-\frac{1}{3}}\Omega^{-\frac{1}{3}}\right\} \quad . \quad . \quad . \quad (1).$$

Again, the kinetic energy of orbital motion is

$$\tfrac{1}{2}M\left(\frac{Mr}{M+m}\right)^2\Omega^2+\tfrac{1}{2}m\left(\frac{Mr}{M+m}\right)^2\Omega^2=\tfrac{1}{2}\frac{Mm}{M+m}r^2\Omega^2=\tfrac{1}{2}\mu^{\frac{1}{3}}Mm(M+m)^{-\frac{1}{3}}\Omega^{\frac{2}{3}}.$$

The kinetic energy of the planet's rotation is $\frac{1}{2}Cn^2$.

The potential energy of the system is

$$-\mu\frac{Mm}{r}=-\mu^{\frac{1}{3}}Mm(M+m)^{-\frac{1}{3}}\Omega^{\frac{2}{3}}.$$

Adding the three energies together

$$2e=C\left\{n^2-\mu^{\frac{1}{3}}\frac{Mm}{C}(M+m)^{-\frac{1}{3}}\Omega^{\frac{2}{3}}\right\} \quad . \quad . \quad . \quad (2).$$

Now, suppose that by a proper choice of the unit of time, $\mu^{\frac{1}{3}}\frac{Mm}{C}(M+m)^{-\frac{1}{3}}$ is unity, and that by a proper choice of units of length or of mass C is unity,* and

Let $\qquad x=\Omega^{-\frac{1}{3}}, \qquad y=n, \qquad Y=2e.$

* Let $\nu=\frac{M}{m}$, then if g be the mean gravity at the surface of the planet, and if a be its mean radius,

$$\mu(M+m)=ga^2\frac{1+\nu}{\nu}$$

and
$$\mu^{\frac{1}{3}}Mm(M+m)^{-\frac{1}{3}}=\left[ga^2\frac{1+\nu}{\nu}\right]^{\frac{1}{3}}\frac{M}{1+\nu}=Ma^2\div\left\{\left(\frac{a\nu}{g}\right)^2(1+\nu)\right\}^{\frac{1}{3}}$$

Then if the planet be homogeneous, and differ infinitesimally from a sphere, $C=\frac{2}{5}Ma^2$, and;

It may be well to notice that x is proportional to the square root of the satellite's distance from the planet.

Then the equations (1) and (2) become

$$h=y+x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

$$Y=y^2-\frac{1}{x^2}=(h-x)^2-\frac{1}{x^2} \quad . \quad . \quad . \quad . \quad . \quad (4).$$

(3) is the equation of conservation of moment of momentum, or shortly, the equation of momentum; (4) is the equation of energy.

Now, consider a system started with given positive (or say clockwise) moment of momentum h; we have all sorts of ways in which it may be started. If the two rotations be of opposite kinds, it is clear that we may start the system with any amount of energy however great, but the true maxima and minima of energy compatible with the given moment of momentum are given by $\frac{dY}{dx}=0$,

or
$$x-h+\frac{1}{x^3}=0,$$

or
$$x^4-hx^3+1=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

We shall presently see that this biquadratic has either two real roots and two imaginary, or all imaginary roots.

This biquadratic may be derived from quite a different consideration, viz., by finding the condition under which the satellite may move round the planet, so that the planet shall always show the same face to the satellite, in fact, so that they move as parts of one rigid body.

The condition is simply that the satellite's orbital angular velocity

$$\mu^{\frac{1}{2}}\frac{Mm}{C}(M+m)^{-\frac{1}{2}}=1\div\frac{2}{5}\left\{\left(\frac{a\nu}{g}\right)^2(1+\nu)\right\}^{\frac{1}{3}}=\frac{1}{s}, \text{ suppose}$$

in the case of the earth, considered as heterogeneous, the $\frac{2}{5}$ would be replaced by about $\frac{1}{3}$.

It is clear that $s^{\frac{3}{2}}$ is a time; and in the case of the earth and moon (with $\nu=82$),

$s^{\frac{3}{2}}=3$ hrs. $4\frac{1}{2}$ mins., if the earth be homogeneous, and

$s^{\frac{3}{2}}=2$ hrs. 41 mins. if the earth be heterogeneous.

For the units of length and mass we have only to choose them so that $\frac{2}{5}Ma^2$, or $\frac{1}{3}Ma^2$, may be unity.

With these units it will be found that for the present length of day $n=\cdot8056$ (homog.) or $\cdot7026$ (heterog.), and that

$$h=\cdot8056[1+4\cdot01]=4\cdot03 \text{ (homog.)},$$

$$\text{or } h=\cdot7026[1+4\cdot38]=3\cdot78 \text{ (heterog.)}$$

For the value 4·38 see Thomson and Tait's "Nat. Phil.," § 276, where tidal friction is considered.

$\Omega=n$ the planet's angular velocity round its axis; or since $n=y$ and $\Omega^{-\frac{1}{2}}=x$, therefore $y=\frac{1}{x^3}$.

By substituting this value of y in the equation of momentum (3), we get as before

$$x^4-hx^3+1=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

In my paper on the "Precession of a Viscous Spheroid,"* I obtained the biquadratic equation from this last point of view only, and considered analytically and numerically its bearings on the history of the earth.

Sir William Thomson, having read the paper, told me that he thought that much light might be thrown on the general physical meaning of the equation, by a comparison of the equation of conservation of moment of momentum with the energy of the system for various configurations, and he suggested the appropriateness of geometrical illustration for the purpose of this comparison. The method which is worked out below is the result of the suggestions given me by him in conversation.

The simplicity with which complicated mechanical interactions may be thus traced out geometrically to their results appears truly remarkable.

At present we have only obtained one result, viz.: that if with given moment of momentum it is possible to set the satellite and planet moving as a rigid body, then it is possible to do so in two ways, and one of these ways requires a maximum amount of energy and the other a minimum; from which it is clear that one must be a rapid rotation with the satellite near the planet, and the other a slow one with the satellite remote from the planet.

Now, consider the three equations,

$$h=y+x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

$$Y=(h-x)^2-\frac{1}{x^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

$$x^3y=1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

(6) is the equation of momentum; (7), that of energy; and (8) we may call the equation of rigidity, since it indicates that the two bodies move as though parts of one rigid body.

Now, if we wish to illustrate these equations geometrically, we may take as abscissa x, which is the m. of m. of orbital motion; so that the axis of x may be called the axis of orbital momentum. Also, for equations (6) and (8) we may take as ordinate y, which is the m. of m. of the planet's rotation; so that the axis of y may be called the axis of rotational momentum. For (7) we may take as ordinate Y,

* Of which an abstract appears in "Proc. Roy. Soc.," No. 191, 1878.

which is twice the energy of the system; so that the axis of Y may be called the axis of energy. Then, as it will be convenient to exhibit all three curves in the same figure, with a parallel axis of x, we must have the axis of energy identical with that of rotational momentum.

It will not be necessary to consider the case where the resultant m. of m. h is negative, because this would only be equivalent to reversing all the rotations; thus h is to be taken as essentially positive.

Then the line of momentum, whose equation is (6), is a straight line at 45° to either axis, having positive intercepts on both axes.

The curve of rigidity, whose equation is (8), is clearly of the same nature as a rectangular hyperbola, but having a much more rapid rate of approach to the axis of orbital momentum than to that of rotational momentum.



The intersections (if any) of the curve of rigidity with the line of momentum have abscissæ which are the two roots of the biquadratic $x^4-hx^3+1=0$. The biquadratic has, therefore, two real roots or all imaginary roots. Then, since $x=\Omega^{-\frac{1}{3}}$, it varies as $\sqrt{r}$, and, therefore, the intersection which is more remote from the origin, indicates a configuration where the satellite is remote from the planet; the other



gives the configuration where the satellite is closer to the planet. We have already learnt that these two correspond respectively to minimum and maximum energy.

When x is very large, the equation to the curve of energy is $Y=(h-x)^2$, which is the equation to a parabola, with a vertical axis parallel to Y and distant h from the origin, so that the axis of the parabola passes through the intersection of the line of momentum with the axis of orbital momentum.

When x is very small the equation becomes $Y=-\frac{1}{x^2}$.

Hence, the axis of Y is asymptotic on both sides to the curve of energy.

Then, if the line of momentum intersects the curve of rigidity, the curve of energy has a maximum vertically underneath the point of intersection nearer the origin, and a minimum underneath the point more remote. But if there are no intersections, it has no maximum or minimum.

It is not easy to exhibit these curves well if they are drawn to scale, without making a figure larger than it would be convenient to print, and accordingly fig. 1 gives them as drawn with the free hand. As the zero of energy is quite arbitrary, the origin for the energy curve is displaced downwards, and this prevents the two curves from crossing one another in a confusing manner. The same remark applies also to figs. 2 and 3.

Fig. 1 is erroneous principally in that the curve of rigidity ought to approach its horizontal asymptote much more rapidly, so that it would be difficult in a drawing to scale to distinguish the points of intersection B and D.

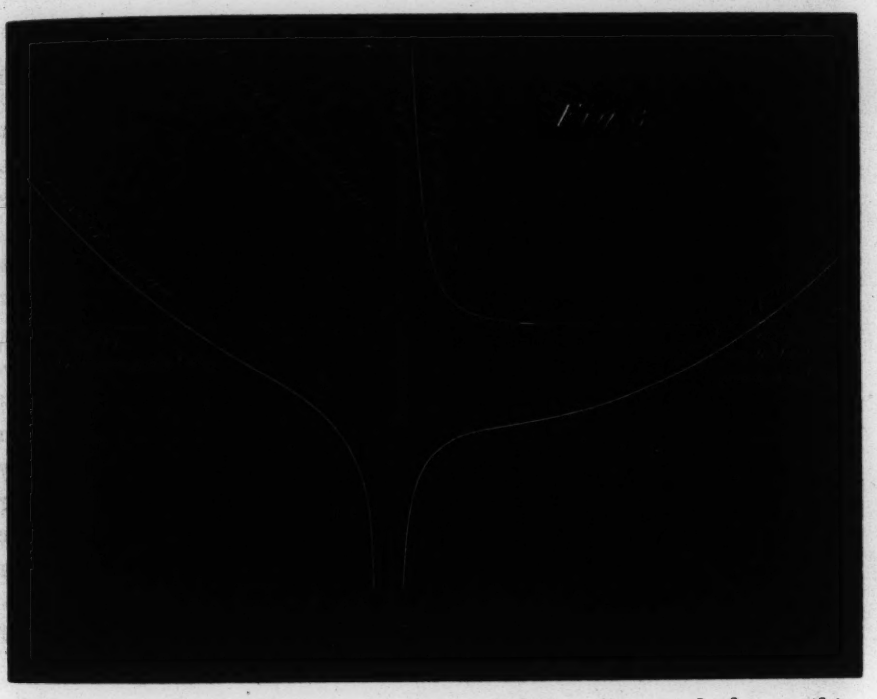
Fig. 2 exhibits the same curves, but drawn to scale, and designed to be applicable to the case of the earth and moon, that is to say, when $h=4$ nearly.

Fig. 3 shows the curves when $h=1$, and when the line of momentum does not intersect the curve of rigidity; and here there is no maximum or minimum in the curve of energy.

These figures exhibit all the possible methods in which the bodies may move with given moment of momentum, and they differ in the fact that in figs. 1 and 2 the biquadratic (5) has real roots, but in the case of fig. 3 this is not so. Every point of the line of momentum gives by its abcissa and ordinate the square root of the satellite's distance and the rotation of the planet, and the ordinate of the energy curve gives the energy corresponding to each distance of the satellite.

Parts of these figures have no physical meaning, for it is impossible for the satellite to move round the planet at a distance which is less than the sum of the radii of the planet and satellite. Accordingly in

fig. 1 a strip is marked off and shaded on each side of the vertical axis, within which the figure has no physical meaning.



Since the moon's diameter is about 2,200 miles, and the earth's about 8,000, therefore the moon's distance cannot be less than 5,100 miles; and in fig. 2, which is intended to apply to the earth and moon and is drawn to scale, the base of the strip is only shaded, so as not to render the figure confused. The strip has been accidentally drawn a very little too broad.

The point P in fig. 2 indicates the present configuration of the earth and moon.

The curve of rigidity $x^3y=1$ is the same for all values of h, and by moving the line of momentum parallel to itself nearer or further from the origin, we may represent all possible moments of momentum of the whole system.

The smallest amount of m. of m. with which it is possible to set the system moving as a rigid body, is when the line of momentum touches the curve of rigidity. The condition for this is clearly that the equation $x^4-hx^3+1=0$ should have equal roots. If it has equal roots each root must be $\frac{3}{4}h$, and therefore

$$(\tfrac{3}{4}h)^4-h(\tfrac{3}{4}h)^3+1=0.$$

whence $h^4=\frac{4^4}{3^3}$ or $h=\frac{4}{3^{\frac{3}{4}}}=1{\cdot}75$.

The actual value of h for the moon and earth is about $3\frac{3}{4}$, and hence

if the moon-earth system were started with less than $\frac{7}{15}$ of its actual moment of momentum, it would not be possible for the two bodies to move so that the earth should always show the same face to the moon.

Again if we travel along the line of momentum there must be some point for which yx^3 is a maximum, and since $yx^3=\frac{n}{\Omega}$ there must be some point for which the number of planetary rotations is greatest during one revolution of the satellite, or shortly there must be some configuration for which there is a maximum number of days in the month.

Now yx^3 is equal to $x^3(h-x)$, and this is a maximum when $x=\frac{3}{4}h$ and the maximum number of days in the month is $(\frac{3}{4}h)^3\ (h-\frac{3}{4}h)$ or $\frac{3^3}{4^4}h^4$; if h is equal to 4, as is nearly the case for the homogeneous earth and moon, this becomes 27.

Hence it follows that we now have very nearly the maximum number of days in the month. A more accurate investigation in my paper on the "Precession of a Viscous Spheroid," showed that taking account of solar tidal friction and of the obliquity to the ecliptic the maximum number of days is about 29, and that we have already passed through the phase of maximum.

We will now consider the physical meaning of the several parts of the figures.

It will be supposed that the resultant moment of momentum of the whole system corresponds to a clockwise rotation.

Now imagine two points with the same abscissa, one on the momentum line and the other on the energy curve, and suppose the one on the energy curve to guide that on the momentum line.

Then since we are supposing frictional tides to be raised on the planet, therefore the energy must degrade, and however the two points are set initially, the point on the energy curve must always slide down a slope carrying with it the other point.

Now looking at fig. 1 or 2, we see that there are four slopes in the energy curve, two running down to the planet, and two others which run down to the minimum. In fig. 3 on the other hand there are only two slopes, both of which run down to the planet.

In the first case there are four ways in which the system may degrade, according to the way it was started; in the second only two ways.

i. Then in fig. 1, for all points of the line of momentum from C through E to infinity, x is negative and y is positive; therefore this indicates an anti-clockwise revolution of the satellite, and a clockwise rotation of the planet, but the m. of m. planetary rotation is greater than that of the orbital motion. The corresponding part of the curve of energy slopes uniformly down, hence however the system be started,

for this part of the line of momentum, the satellite must approach the planet, and will fall into it when its distance is given by the point k.

ii. For all points of the line of momentum from D through F to infinity, x is positive and y is negative; therefore the motion of the satellite is clockwise, and that of the planetary rotation anti-clockwise, but the m. of m. of the orbital motion is greater than that of the planetary rotation. The corresponding part of the energy curve slopes down to the minimum b. Hence the satellite must approach the planet until it reaches a certain distance where the two will move round as a rigid body. It will be noticed that as the system passes through the configuration corresponding to D, the planetary rotation is zero, and from D to B the rotation of the planet becomes clockwise.

If the total moment of momentum had been as shown in fig. 3, then the satellite would have fallen into the planet, because the energy curve would have no minimum.

From i and ii we learn that if the planet and satellite are set in motion with opposite rotations, the satellite will fall into the planet, if the moment of momentum of orbital motion be less than or equal to or only greater by a certain critical amount,* than the moment of momentum of planetary rotation, but if it be greater by more than a certain critical amount the satellite will approach the planet, the rotation of the planet will stop and reverse, and finally the system will come to equilibrium when the two bodies move round as a rigid body, with a long periodic time.

iii. We now come to the part of the figure between C and D. For the parts AC and BD of the line AB in fig. 1, the planetary rotation is slower than that of the satellite's revolution, or the month is shorter than day, as in one of the satellites of Mars. In fig. 3 these parts together embrace the whole. In all cases the satellite approaches the planet. In the case of fig. 3, the satellite must ultimately fall into the planet; in the case of figs. 1 and 2 the satellite will fall in if its distance from the planet is small, or move round along with the planet as a rigid body if its distance be large.

For the part of the line of momentum AB, the month is longer than the day, and this is the case of all known satellites except the nearer one of Mars. As this part of the line is non-existent in fig. 3, we see that the case of all existing satellites (except the Martian one) is comprised within this part of figs. 1 and 2. Now if a satellite be placed in the condition A, that is to say, moving rapidly round a planet, which always shows the same face to the satellite, the condition is clearly dynamically unstable, for the least disturbance will determine whether the system shall degrade down the slopes ac or ab, that is to say, whether it falls into or recedes from the planet. If

* With the units which are here used the excess must be more than $4 \div 3\frac{3}{4}$; see further back.

the equilibrium breaks down by the satellite receding, the recession will go on until the system has reached the state corresponding to B.

The point P, in fig. 2, shows approximately the present state of the earth and moon, viz., when $x=3{\cdot}2$, $y={\cdot}8$.

It is clear that, if the point l, which indicates that the satellite is just touching the planet, be identical with the point A, then the two bodies are in effect part of a single body in an unstable configuration. If, therefore, the moon was originally part of the earth, we should expect to find A and l identical. The figure 2, which is drawn to represent the earth and moon, shows that there is so close an approach between the edge of the shaded band and the intersection of the line of momentum and curve of rigidity, that it would be scarcely possible to distinguish them on the figure. Hence, there seems a considerable probability that the two bodies once formed parts of a single one, which broke up in consequence of some kind of instability. This view is confirmed by the more detailed consideration of the case in the paper on the "Precession of a Viscous Spheroid," of which only an abstract has as yet been printed.

Hitherto the satellite has been treated as an attractive particle, but the graphical method may be extended to the case where both the satellite and planet are spheroids rotating about axes perpendicular to the plane of the orbit.

Suppose, then, that k is the ratio of the moment of inertia of the satellite to that of the planet, and that z is equal to the angular velocity of the satellite round its axis, then kz is the moment of momentum of the satellite's rotation, and we have

$$h=x+y+kz \text{ for the equation to the plane of momentum,}$$

$$2e=y^2+kz^2-\frac{1}{x^2} \text{ for the equation of energy,}$$

and $x^3y=1$, $x^3z=1$ for the equation to the line of rigidity.

The most convenient form in which to put the equation to the surface of energy is

$$\mathrm{E}=y^2+kz^2-\frac{1}{(h-y-kz)^2},$$

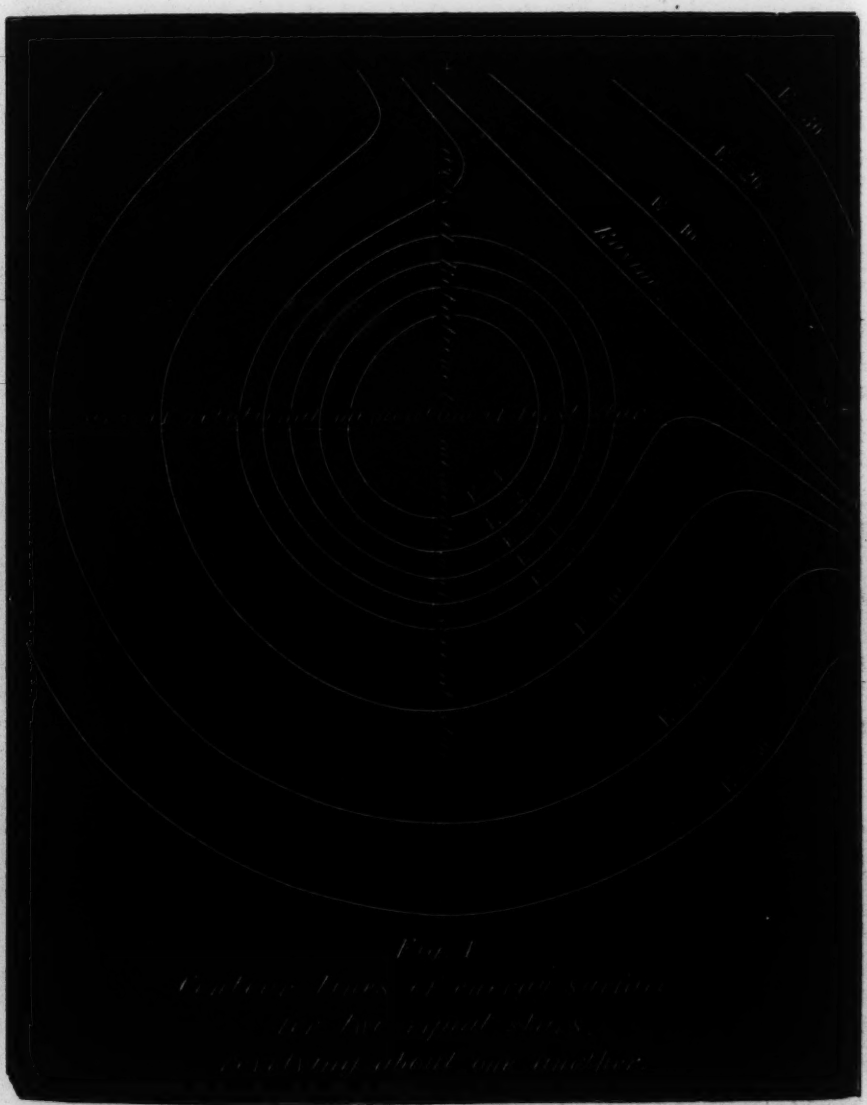
where E, y, z are the three ordinates.

The best way of understanding the surface is to draw the contour-lines of energy parallel to the plane of yz, as shown in fig. 4.

The case which I have considered may be called a double-star system, where the planet and satellite are equal and $k=1$. Any other case may be easily conceived by a stretching or contraction of the surface parallel to z.

It will be found that, if the whole moment of momentum h has less than a certain critical value (found by the consideration that

$x^4-hx^3+2=0$ has equal roots), then the surface may be conceived as an infinitely narrow and deep ravine, opening out at one part of its course into rounded valleys on each side of the ravine. In this case the contours would resemble those of fig. 4, supposing the round closed curves to be absent. The course of the ravine is at 45° to the axes of y and z, and the origin is situated in one of the valleys, which is less steep than the valley facing it on the opposite side of the ravine. The form of a section perpendicular to the ravine is such as the curve of energy in fig. 3, so that everywhere there is a slope towards the ravine.



Every point on the surface corresponds to one configuration of the system, and, if the system be guided by a point on the energy surface,

that point must always slide down hill. It does not, however, necessarily follow that it will always slide down the steepest path. The fall of the guiding point into the ravine indicates the falling together of the two stars.

Thus, if the two bodies be started with less than a certain moment of momentum, they must ultimately fall together.

Next, suppose the whole m. of m. of the system to be greater than the critical value. Now, the less steep of the two valleys of the former case (viz., the one in which the origin lies) has become more like a semicircular amphitheatre of hills, with a nearly circular lake at the bottom; and the valley facing the amphitheatre has become merely a falling back of the cliffs which bound the ravine. The energy curve in fig. 2 would show a section perpendicular to the ravine through the middle of the lake.

The origin is nearly in the centre of the lake, but slightly more remote from the ravine than the centre.

In this figure h was taken as 4, and k as unity, so that it represents a system of equal double stars. The numbers written on each contour give the value of E corresponding to that contour.

Now, the guiding point of the system, if on the same side of the ravine as the origin, may either slide down into the lake or into the ravine. If it falls into the ravine, the two stars fall together, and if to the bottom of the lake, the whole system moves round slowly, like a rigid body.

If the point be on the lip of the lake, with the ravine on one side and the lake on the other, this corresponds to the motion of the two bodies rapidly round one another, moving as a rigid body; and this state is clearly dynamically unstable.

If the point be on the other side of the ravine, it must fall into it, and the two stars fall together.

It has been remarked that the guiding point does not necessarily slide down the steepest gradient, and of such a mode of descent illustrations will be given hereafter.

Hence it is possible that, if the guiding point be started somewhere on the amphitheatre of hills, it may slide down until it comes to the lip of the lake. As far as one can see, however, such a descent would require a peculiar relationship of the viscosities of the two stars, probably varying from time to time. It is therefore possible, though improbable, that the unstable condition where the two bodies move rapidly round one another, always showing the same faces to one another, may be a degradation of a previous condition. If this state corresponds with a distance between the stars less than the sum of the radii of their masses, it clearly cannot be the result of such a degradation.

If, therefore, we can trace back a planet and satellite to this state,

we have most probably found the state where the satellite first had a separate existence.

The conditions of stability of a rotating mass of fluid are very obscure, but it seems probable that, if the stability broke down and the mass gradually separated into two parts, then the condition immediately after separation might be something like the unstable configuration described above.

In conclusion, I will add a few words to show that the guiding point on an energy surface need not necessarily move down the steepest path, but may even depart from the bottom of a furrow or move along a ridge. Of this two cases will be given.

The satellite will now be again supposed to be merely an attractive particle.

First, with given moment of momentum, the energy is greater when the axis of the planet is oblique to the orbit. Hence, if we draw an energy surface in which one of the co-ordinate axes corresponds to obliquity, then there must be a furrow in the surface corresponding to zero obliquity. To conclude that the obliquity of the ecliptic must diminish in consequence of tidal friction would be erroneous. In fact, it will appear, in my paper on the "Precession of a Viscous Spheroid," that for a planet of small viscosity the position of zero obliquity is dynamically unstable, if the period of the satellite is greater than twice that of the planet's rotation. Thus the guiding point, though always descending on the energy surface, will depart from the bottom of the furrow.

Secondly. For given moment of momentum the energy is less if the orbit be eccentric, and an energy surface may be constructed in which zero eccentricity corresponds to a ridge. Now, I believe that I shall be able to show, in a future paper, that for small viscosity of the planet the circular orbit is dynamically stable if eighteen periods of the satellite be less than eleven periods of the planet's rotation. This will afford a case of the guiding point sliding down a ridge; when, however, the critical point is passed, the guiding point will depart from the ridge and the orbit become eccentric.

IX. "Researches in Chemical Equivalence. Part III.* Nickelous and Cobaltous Sulphates." By Edmund J. Mills, D.Sc., F.R.S., and J. J. Smith. Received June 2, 1879.

Although the chemistry of nickel and cobalt is interesting from many points of view, it is more especially attractive from the probable isomerism of these metals. Their combining proportions, in fact,

* For Part II, see "Proceedings," vol. xxviii, p. 270.

according to the most valuable evidence we possess,* appear to be entirely the same. We, therefore, thought it very advisable to inquire on what terms they might prove to be mutually equivalent: and the particular equivalence we have examined has been equivalent precipitability of the sulphates, by sodic hydrate, from an aqueous solution.

I. *Preparation of the Salts.*

The pure cobaltous salt was prepared by converting some excellent commercial crystallized chloride into luteochloride, the process employed having been already described by one of us.† The luteochloride was purified by precipitation with hydric chloride, and the mixture of oxides it left behind on ignition was evaporated with redistilled hydric sulphate.

The pure nickelous salt was prepared from a sample of nickelous chloride which contained copper, lime, and iron, but no cobalt. The copper present was precipitated with hydric sulphide, and the nickel in the filtrate was precipitated by hydric oxalate, in an acid solution. The nickelous oxalate was washed thoroughly with dilute hydric nitrate, ignited, and the oxide so formed heated with pure hydric sulphate, and so converted into sulphate.

II. *Method of separating Nickel from Cobalt.*

We had next to select a method for the quantitative separation of nickel from cobalt. The first to which we had recourse was Liebig's,‡ which consists in adding hydric cyanide and potash to the mixed saline solutions, thereby forming nickelopotassic cyanide and potassic cobalticyanide: these new compounds are boiled with freshly precipitated mercuric oxide, which throws down the nickel as oxide and cyanide. The cobalt in the filtrate is then precipitated as mercurous cobalticyanide. On the ignition of these precipitates, nickelous oxide and cobaltic oxide are respectively left behind. We converted the oxides into sulphates and weighed them as such. (Throughout the whole of these experiments the nickel and cobalt were always weighed as sulphates.) We found, however, that after the expulsion, by ignition, of the mercury in the precipitates, the oxides were left in so dense and compact a state, that it was only with the greatest difficulty that they could be converted into sulphates. To remedy this, a weighed quantity of pure baric sulphate was added to the liquid before precipitation; the precipitate, becoming mixed with the baric sulphate, was thus spread over a large surface, so that the oxide was obtained in a finely divided state, and easily converted into sulphate.

* Russell, "Journal of the Chemical Society," (1869), p. 294.

† "Philosophical Magazine," (4), xxxv, p. 245.

‡ "Ann. Ch. Pharm.," lxv, 244.

In the experiments with this process, the nickel was invariably found too high. This was at first thought to be due to alkali which might cling to the precipitated oxide; and, on testing, some potash was actually found in the precipitate, which had been thoroughly washed. To remove this source of error, the nickelous sulphate, instead of being weighed as such, was dissolved in water, excess of ammonia added, and the nickel deposited on a platinum crucible, by means of two Grove's cells.

But the nickel was, with this method, still found too high, and we then thought that the separation had not been perfect. On testing the deposit, a considerable amount of cobalt was, in fact, found in it, and the operation required to be repeated three times before the separation was complete.

The next process we tried was devised by Rose.* It consists in saturating with chlorine a very dilute solution of the mixed salts, adding baric carbonate in excess, and allowing to stand from twelve to eighteen hours, with occasional shaking. The cobalt then falls as sesquioxide, the nickel remaining in solution.

But the results obtained by this method, as was also the case with Henry's modification of it (which consists in substituting bromine for chlorine) were very variable. In the former case the cobalt was generally found too low, even after standing over eighteen hours; the result appearing to depend a good deal on the liquid being shaken up at regular intervals, which cannot be very conveniently done, during an entire period of eighteen hours. In the latter case, the cobalt was generally found too high, nickel being precipitated along with it.

The last method tried, and the one finally adopted, is due to Gibbs.† In this, the neutral solution of the sulphates is boiled with plumbic peroxide. The cobalt is then precipitated as a higher oxide, while the nickel remains in solution, along with a small quantity of lead. (The author of this process does not claim very great accuracy for it, but we have found it to be of adequate accuracy.)

The manner in which we operated was as follows:—The perfectly neutral solution of the mixed sulphates, was boiled for half an hour with plumbic peroxide, about 7 grms. of the peroxide being taken to 1 grm. of cobaltous sulphate, and the volume of the liquid being about 100 cub. centims. The liquid was then filtered, and the filtrate evaporated to about 20 cub. centims. Some aqueous hydric sulphide was then added, and the small quantity of plumbic sulphide formed filtered off. The solution of nickelous sulphate was then evaporated to dryness in a weighed crucible, ignited, treated with a little hydric sulphate, heated to very dull redness and weighed. The precipitate containing the cobalt, with excess of plumbic peroxide and some sul-

* "Pogg. Ann.," lxxi, 545.

† "Sill. Am. J.," xiv, 295.

phate, was boiled with hydric chloride until dissolved. Water was then added, and the lead precipitated with hydric sulphate and alcohol. The filtrate from the plumbic sulphate was then evaporated to dryness, heated till the excess of hydric sulphate was driven off, and dissolved in water. A little hydric sulphide was then added, to remove a small quantity of lead still present, the filtrate evaporated to dryness, and the cobaltous sulphate weighed. The plumbic peroxide must, of course, be perfectly pure, because any impurity in it finds its way into the cobalt, and consequently makes that result too high.

In an experiment with this process, ·2500 grm. of cobaltous sulphate, and ·2500 grm. nickelous sulphate were taken, and the quantities found after the separation were ·2505 grm. cobaltous sulphate, and ·2490 grm. nickelous sulphate. The estimation of the cobalt is rather a troublesome and tedious process; but when the mixture of nickel and cobalt can be weighed, and the nickel in it estimated, the cobalt being taken by difference, this is an admirable method to employ. The following are some results of the estimation of the nickel in a mixture of ·2500 grm. of cobaltous sulphate and ·2500 grm. of nickelous sulphate.

	Nickelous sulphate taken.	Found.
(1.)	·2500	·2480
(2.)	„	·2505
(3.)	„	·2490
(4.)	„	·2495
(5.)	„	·2480
	Mean....	·2490

Probable error of a single determination 0·16 per cent.

III. *Experiments on Equivalence.*

Having then fixed on the method of separation, 1 per cent. solutions of nickelous and cobaltous sulphates were prepared, and a solution of sodic hydrate, of which 10 cub. centims. were capable of precipitating ·8248 grm. of nickelous or cobaltous sulphate. This sodic hydrate was made from sodium, and kept in glass bottles coated internally with a thick layer of paraffin.

A series of nine experiments was made, in which the relative weights of nickelous or cobaltous sulphate present, varied from ·1 to ·9 grm.; the total weight of nickelous and cobaltous salt, and the volume of the solution being, however, always the same, viz., 1 grm. and 100 cub. centims. The experiments were conducted as follows:— The bottles containing the solutions of the sulphates and the sodic hydrate were immersed in a trough into which there was a constant flow of water to bring them to a constant temperature. The necessary

quantities of nickelous and cobaltous solutions were then carefully measured out, mixed, and the temperature observed. 10 cub. centims. of sodic hydrate were then added, the solution stirred vigorously, and the temperature again observed. The precipitate was then filtered off as quickly as possible (an aspirator being used to facilitate the filtration), and washed, first with cold and then with hot water. Three days' intermittent washing was required to free the precipitate from the undecomposed nickelous and cobaltous salts, cobaltous hydrate having, as is well known, a powerful attraction for cobaltous sulphate, thereby forming a basic salt. After washing, the precipitate was converted into sulphate by treatment with hydric sulphate, and weighed. The mixed sulphates were then dissolved in water, and separated by Gibbs's method, the nickelous sulphate being weighed, and the cobaltous sulphate obtained by difference. It was found that a small quantity of sodic sulphate was always present in the precipitate, the washing having failed to remove it, and this required to be estimated and deducted. For this purpose the nickelous sulphate, after being weighed, was dissolved in water, and the nickel precipitated with baric hydrate. The barium in the filtrate was then removed with hydric sulphate, and the filtrate containing the sodic sulphate evaporated to dryness and weighed. The results obtained are comprised in the following table.

Nickelous salt taken.	Cobaltous salt taken.	Cobaltous salt taken (corrd.)	Total precipitate.	Nickelous salt precipitated.	Cobaltous salt precipitated.	Cobaltous salt precipitated (corrd.)	Sodic sulphate precipitated.	Temperatures.	Number of experiment.
grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	° °	
·1	·9	·8093	·9155	·1050	·8105	·7198	·0105	7–8	I
·2	·8	·7581	·8667	·1980	·6687	·6268	·0110	6–7	II
·3	·7	·6268	·8980	·2765	·6215	·5483	·0135	7–8	III
·4	·6	·5598	·8650	·3510	·5140	·4738	·0105	7–8	IV
·5	·5	·4363	·8885	·4465	·4420	·3783	·0090	7–8	V
·6	·4	·3613	·8835	·5295	·3340	·2953	·0150	7–7·5	VI
·7	·3	·2808	·8440	·5825	·2615	·2423	·0110	7–7·5	VII
·8	·2	·1408	·8840	·7080	·1760	·1168	0130	8–8·5	VIII
·9	·1	·0483	·8765	·7865	·0900	·0383	0105	7–7·5	IX

In this table the precipitates are all returned as sulphates. The total possible amount of normal sulphate attainable with the constant quantity of sodic hydrate employed having been ·8248 grm., we are able to calculate the entire composition of the precipitate thus:—From its total weight, the amount of sodic sulphate is first subtracted; from the residue the constant quantity ·8248; the remainder, which is the cobaltous sulphate carried down with the hydrate, is deducted as a correction from the cobaltous sulphate originally taken and that which was precipitated, it having no share in the reaction we had to examine. Under "temperature" we give the temperatures of the

reagents before and after mixture. The weight of precipitated sodic sulphate varies but little throughout the experiments, its mean value being ·0115 grm. In a special determination, we precipitated, with the usual amount of sodic hydrate, 1·000 grm. of nickelous sulphate alone: after three days' washing, it was found to contain ·0115 grm. of sodic sulphate. Hence we infer that when the two sulphates are present together, nickelous hydrate carries down sodic sulphate, cobaltous hydrate carrying down cobaltous sulphate.

IV. *Discussion.*

In the following discussion, it will be understood that we refer to the tabular results already given.

If n represent a weight of nickelous sulphate taken, and ν be the hydrate (calculated to sulphate) obtained from it through precipitation, then we consider ϕ in the expression $n=\phi\nu$ to represent the *precipitability* of nickelous sulphate: and similarly in $c=\phi\gamma$, ϕ represents the precipitability of cobaltous sulphate. In examining the numbers obtained with the nickelous salt, the best expression we could find for ϕ was $\phi=(a+\beta n)$. We first calculated the values of a and βn from all the determinations, and by the method of least squares, thus obtaining

$$\phi=\cdot98891+\cdot22571n.$$

It is, however, clear that a cannot be less than unity; moreover, the first weight of nickelous precipitate is somewhat higher than is possible, and we have thought well to reject it. With these amendments we finally obtain

$$\phi=1+\cdot21940n,$$

with a probable error of ·02558 for a single determination, or ·00904 for eight determinations. Hence we infer that the *precipitability of nickelous sulphate is directly proportional to its mass.*

In the case of cobaltous sulphate, on the other hand, no such law holds good. After a very careful examination of the numbers, we could not find any satisfactory evidence of a change in precipitability with its mass, and consequently represent ϕ as a constant. The mean value of ϕ is in this case 1·1845, with a probable error of ·02792 on a single determination, or ·00931 for nine determinations. Thus ϕ is about equally well ascertained in both cases.

Our two equations may now be written

$$\left.\begin{aligned} n&=(+\cdot21940\,n)\nu \\ c&=1\cdot1845\gamma \end{aligned}\right\}.$$

In order to calculate in what proportions the two sulphates are equally precipitable, we have

$$1+\cdot21940n=1\cdot1845,$$

whence $n=\cdot84093$. The two sulphates, then, are equally precipitable when the weight taken of each is ·84093 grm.

To calculate in what proportions the two sulphates must be mixed, to give equal weights of precipitate, we have

$$\left.\begin{aligned}\frac{n}{c}&=\frac{(1+\cdot21940\,n)\,\nu}{1\cdot1845\gamma}\\ n&=1-c\end{aligned}\right\}.$$

If we put $\frac{\nu}{\gamma}=1$, and combine these equations, the result is a quadratic, one of whose solutions is $c=\cdot5168$, and consequently $n=\cdot4832$. Using these values in the primitive equations, the results are, $\gamma=\cdot4363$, $\nu=\cdot4369$, which we may regard as substantially equal.

A highly important chemical relation is disclosed when the two sulphates are so conditioned as to be equally precipitable. We have seen that this is the case when $n=c=\cdot84093$. Now the reciprocal of ·84093 is 1·1892,—a number differing by only the small amount of 0·40 per cent. from 1·1845, or $\phi\gamma$. Deduced, as these values both are, from a series of experiments, we cannot hold their connexion to be accidental. It may be thus expressed in symbols,—

$$n=\frac{1}{\phi\gamma}=\frac{\gamma}{c};$$

or we may state the relation thus:—*For an equal weight, nickelous and cobaltous sulphates are equally precipitable; the attraction of the one towards the reagent being then inverse to that of the other.*

In order to ascertain whether this function admits of extension to other weights than ·84093 grm. of the sulphates, we doubled all the masses in one of our previous experiments (V in the table), and determined the chemical effect. The results were

Uncorrected total precipitate	1·7840	grm.
Containing of nickelous sulphate ..	0·8925	„
The halves of these are	0·8920	„
	0·4463	„
Figures nearly identical with.....	0·8975	„
	0·4465	„

derived from Experiment V. By doubling the mass, we thus produce double the chemical effect. It is probable that this law is general; and, therefore, we infer that the reciprocal function we have noticed may apply to multiples of the weights to which, in our particular case, it specially appertained. Bearing in mind that $NiSO_4=CoSO_4=154\cdot65$, we accordingly write the function thus:

$$\phi(NiSO_4)=(\phi CoSO_4)^{-1}.$$

If we imagine some reaction—such, for example, as the combination of colouring matter with a tissue—influenced in one set of experiments by nickelous sulphate, and in an altogether different set by an equal weight of cobaltous sulphate, we can hardly conceive any ground for the development of a reciprocal function, such as we have experimentally traced. On the other hand, it seems reasonable to suppose that, when two bodies are simultaneously confronted with a single reagent, they both contend for its effect. Thus a chemical antagonism may arise between them by virtue merely of their being together; and thence the reciprocal function. So far as we are aware, the only other chemical function of the kind, hitherto investigated, is to be found in Chižyński's* examination of the partial precipitation, by ammonic phosphate, of mixed calcic and magnesic chlorides. That chemist arrived at the conclusion, for which we consider his evidence to be adequate, that "equal masses of calcic and magnesic chlorides have always equal, but oppositely active, coefficients of affinity."

X. "On the Formation of Hydrocyanic Acid in the Electric Arc." By James Dewar, M.A., F.R.S., Professor of Chemistry to the Royal Institution. Received June 5, 1879.

A series of experiments favouring the conclusion that the so-called carbon lines are invariably associated with the formation of acetylene† induced me to make some experiments to ascertain whether this substance can be extracted from the electric arc, which invariably shows this peculiar spectrum at the positive pole, when it is powerful and occasionally intermittent. For this purpose the carbons were used in the form of tubes, so that a current of air could be drawn by means of an aspirator through either pole, and the products thus extracted from the arc, collected in water, alkalies, and other absorbents. Gases may be led through one of the poles, and suction induced through the other, in order to examine their effect on the arc.

The following experiments record the results obtained by means of the Siemens and de Méritens's magneto-machines.

Experiment 1.—Drew a current of air by an aspirator through the drilled negative carbon, and passed the gases through potash, and iodide of potassium, and starch paste; found no nitrites; potash contained sulphides.

Experiment 2.—Hydrogen led in by the positive pole and the gases extracted as above, gave the well-known acetylene compound with

* Ann. Ch. Pharm., Supp. iv, 226—253.

† As suggested by Plücker, Ångström, and Thalén.

ammoniacal sub-chloride of copper; while, at the same time, a wash-bottle containing water gave distinct evidences of the presence of hydrocyanic acid.

Experiment 3.—Hydrogen flame burning alone gave no sulphides or hydrocyanic acid, the condensed water in a small bulb gave nitrites.

Experiment 4.—Air drawn through the negative carbon gave considerable quantities of hydrocyanic acid, which was greatly increased by extracting the gases through the positive carbon. Air was aspirated at the rate of about one litre per minute.

Experiment 5.—The same carbons used with de Méritens's magneto-machine gave no result.

Experiment 6.—Carbons purified in chlorine and hydrogen gave with de Méritens's nothing; with Siemens' and a draught of air through the negative pole, a small quantity of hydrocyanic acid, but a larger yield when the positive was used. The gases extracted after the absorption of the hydrocyanic acid contained acetylene. If the carbons are not purified, sulphuretted hydrogen is always found along with other gases.

The inference to be drawn from the above experiments is that the high temperature of the positive pole is required to produce the reaction, which is in all probability the result of acetylene reacting with free nitrogen, as when induction sparks are passed through the mixed gases, viz.—

$$C_2H_2+N_2=2HCN,$$

and that the hydrogen is obtained from the decomposition of aqueous vapour, and the combined hydrogen in the carbons. It is possible that traces of alkaline salts in the carbon poles may favour the formation of hydrocyanic acid, but, as all attempts to purify the poles so as to stop the reaction failed, I am inclined to believe it is a direct synthesis. The acetylene reaction is one of the many remarkable syntheses discovered by Prof. Berthelot of Paris. The presence of sulphuretted hydrogen is doubtless due to the reduction of the sulphates, invariably present in the ash of the carbon.

A more complete examination of the various reactions to be brought about by means of the Siemens arc, and with poles of varied composition, and in presence of different gases, will be communicated to the Society in a subsequent paper.

My thanks are especially due to Messrs. Ansdell and Cottrell, assistants in the Royal Institution, for their valuable aid.

XI. "On the Production of Coloured Spectra by Light." By Captain Abney, R.E., F.R.S. Received June 9, 1879.

(Preliminary Note.)

Last year I incidentally mentioned in a note to the Royal Society ("Proceedings," vol. xxviii, p. 291), that the production of natural colours by the agency of light, examples of which were shown by Becquerel, was probably caused by the oxidation of silver compounds employed. I have ventured to return to the subject, in order to show that the colours are so produced and are not due to interference.

I have sent, for the Society's inspection, pictures of the solar spectrum on silver plates, and also on compounds of silver held *in situ* by collodion. It will be observed that the spectrum has imprinted itself in approximafely its natural colours; that on the silver plates it is more brilliant than on the collodion film, but that in the latter the colours are seen by transmitted as well as by reflected light.

I reserve for the present the exact details of the production of these pictures, but may say that they are produced by oxidation of silver compounds when placed in the spectrum; an exposure of two minutes being amply sufficient with a wide slit to impress the colours. The colouring matter seems to be due to a mixture of two different sizes of molecules of the same chemical composition, one of which absorbs at the blue end and the other at the red end of the spectrum, and the sizes of these molecules are unalterable whilst exposed to the same wave-lengths as those by which they were produced. I believe it possible and probable that the colours may be preserved unchanged when exposed to ordinary daylight.

XII. "Relations between the Atomic Weights and certain Physical Properties (Melting and Boiling Points and Heats of Formation) of Elements and Compounds." By Thomas Carnelley, D.Sc., Assistant Lecturer on Chemistry in Owens College, Manchester. Communicated by Professor H. E. Roscoe, F.R.S. Received June 10, 1879.

(Abstract.)

The object of the present paper is to trace the influence of the atomic weights on the melting and boiling points and heats of formation of elements, and especially of their compounds. It is shown that, as regards the elements, the melting points are a periodic function

of their atomic weights, whilst, for compounds, the following conclusions are drawn:—

I. That the melting and boiling points and heats of formation of the normal halogen compounds of the elements are a periodic function of the atomic weights of the constituent elements.

II. That the influence of the halogen on these same physical properties increases with the number of its atoms in the compound.

III. That in any normal halogen compound the influence of either of the elements on the melting or boiling point increases with its own atomic weight, and decreases with the atomic weight of the other element.

IV. The melting or boiling point or heat of formation of a bromide is always nearer to that of the corresponding chloride than to that of the corresponding iodide; and that the melting or boiling points of the halogen compounds of the middle member of three consecutive elements of the same group are always nearer to those of the first member (*i.e.*, the one with least atomic weight) than to those of the last member.

The former of these phenomena probably depends on the fact that the atomic weight of Br is nearer to that of Cl than to that of I, *i.e.*, less than the mean of the two: and the latter on the fact that the atomic weight of the middle member of three consecutive elements of the same group is always less than the mean of those of the other two elements.

V. That the melting and boiling points of the halogen compounds of the elements belonging to the first and second groups of Mendelejeff's classification are widely separated from those of the other groups, being in fact considerably higher. Different relations too often appear to exist between the melting points of the *even* members of these two groups to those which exist between groups (3—7) inclusive; while the compounds of the elements, which are often placed in the *odd* divisions of the first and second groups are generally altogether irregular. In the case of the odd members of the first group this may be explained by the fact that it is very uncertain whether Cu, Ag, and Au really belong to the same group as Na or not, as pointed out by Mendelejeff in his memoir on the "Periodic Law," in which he places these elements not in the first group along with Na, but in the eighth with Fe, Pd, Pt, &c.

As far as existing data allow us to judge, the compounds of the elements with monatomic organic radicals also obey the same laws as those of the halogen compounds.

A process for calculating unknown melting and boiling points (in the case of halogen compounds) by a method of limits is next described, which gives very good results when applied to known melting and boiling points, the average error for *all* known boiling points being

$\pm +4°{\cdot}5$ C., or 0·8 per cent., and for *all* known melting points ± 15, or 3 per cent. The calculated values for a large number of unknown melting and boiling points are given.

It is also shown that a knowledge of the melting or boiling points of the halogen compounds of an element may serve for the determination of the atomic weight when the application of the methods of specific heat and vapour density are inadmissible, or the results obtained by these contradictory.

An application of this is made in the case of beryllium, concerning the atomic weight of which there has recently been some dispute. From his determination of the specific heat of the metal, Emmerson Reynolds concludes that it is a dyad with atomic weight 9·2, whereas Nilson and Petterssen, from their determination of the specific heat, assign to it the atomic weight 13·8, in which case it would be trivalent. Now, according to calculation, the melting points of the chloride, bromide, and iodide of beryllium ought to be (820–870°)* (802–820°) and (766–777°) respectively. The boiling points, also, ought to *diminish* from the chloride to the iodide, if beryllium is a dyad with atomic weight 9·2. If it be a triad with atomic weight 13·8 these melting points ought to be about 500° lower, and the melting and boiling points ought to *increase* from the chloride to the iodide. With the object of settling this point the melting points of the chloride and bromide have been carefully determined, and it was found that the chloride melts at 858–890°, thus agreeing with the limits (820–870°) calculated for $BeCl_2$ (Be=9·2). The bromide also fuses between 858 and 890°, and at almost exactly the same temperature as the chloride, but if anything slightly higher, the calculated number for $BeBr_2$ being 802–820°. The rather high number found for the bromide is probably due to the substance being so readily volatile below its melting point that the heat absorbed during conversion into vapour cools the remainder of the solid, and thus prevents it melting so soon as it otherwise would do. These results confirm the view that beryllium is a dyad, with atomic weight 9·2; for though the melting point of the bromide is *apparently* slightly higher than that of the chloride, for the reason already given, yet $BeBr_2$ is far more volatile than $BeCl_2$.

XIII. "Separate Enumerations of Primes of the Form $4n+1$ and of the Form $4n+3$." By J. W. L. Glaisher, M.A., F.R.S., Fellow of Trinity College, Cambridge. Received June 11, 1879.

The present note contains the results of separate enumerations of primes of the forms $4n+1$ and $4n+3$ for seven groups each of 100,000

* Reckoned from absolute zero -273.

numbers, viz., the subjoined tables give the number of primes of the form $4n+1$ and the number of primes of the form $4n+3$ in each ten thousand of the hundred thousand numbers of the first, second, third, fourth, seventh, eighth, and ninth millions. The properties of the $4n+1$ primes and the $4n+3$ primes are very different; and the idea of separately enumerating the two forms originated with Professor Tchebycheff, and was conveyed to me through M. Edouard Lucas.

The enumerations were effected as follows:—Each page of Burckhardt's and Dase's tables contains eighty lines corresponding to the eighty numbers 1, 7, . . . 97, 101, 103, . . . 199, 203, 209, . . . 299, *i.e.*, to the eighty numbers less than 300, and not divisible by 2, 3, or 5. The lines corresponding to numbers of the form $4n+1$ were ruled through in pencil, and then the bars (representing primes) which were ruled over were enumerated separately, and also the bars which were intact. The enumerations for the first three millions were made from Burckhardt's tables, and those for the seventh, eighth, and ninth millions from Dase's tables. In the case of the fourth million the enumerations were made from the proof sheets of my father's factor table of this million, the whole of which is now printed, and, with the exception of the few last pages, stereotyped.

The results for the groups in the first three millions were communicated to the Meeting of the British Association at Dublin, and are published on p. 471 of the Report for 1878. It is there stated that "the numbers given in the table are the result of a duplicate enumeration; but a third enumeration will be required, in order to render it certain that they are absolutely free from error." This third enumeration has now been made and the following two errors in the British Association table were detected by means of it: in the first ten thousand of the second million the numbers of $4n+1$ and $4n+3$ primes should be respectively 390 and 363, instead of 391 and 362 as printed, and in the third ten thousand of the third million the numbers should be 350 and 343 instead of 349 and 344 as printed. In the case of the other four millions, the results for which are here given for the first time, I have thought a careful duplicate calculation sufficient.

The number of $4n+1$ primes in each hundred was found by counting, and entered in its place on a printed form; and the number of $4n+3$ primes was similarly found and entered on another form. As a verification, the two results for each hundred were added together, and entered on a third form. These totals should agree with the numbers of primes in each hundred, which had been obtained in the previous enumeration that I had made of all the primes in the seven millions; and such was found to be the case. It is to be observed, however, that any error arising from the inaccurate or imperfect ruling of a pencil line would not be detected in this manner, as such an error would consist in a $4n+1$ prime being

counted as a $4n+3$ prime, or *vice versâ;* but pains were taken to verify completely that the pencil lines had been ruled correctly, and for two of the millions (the seventh and ninth) the ruling was performed twice independently, in the one case a second copy of Dase being used, and in the other the original lines having been rubbed out before the second calculation was commenced. The enumerations were however troublesome, and it was much more difficult to ensure freedom from error than in the case of the simple enumerations of primes.

A specimen of a portion of one of the printed forms for the entry of the number of primes in each hundred, which have been referred to above, is given on p. 102 of vol. vii of the "Messenger of Mathematics"* (1877), and a preliminary account of the results of the complete enumeration of all the primes in the first, second, third, seventh, eighth, and ninth millions has been printed in the "Proceedings of the Cambridge Philosophical Society," vol. iii, pp. 17–23, 47–56 (1876–77).† The results of the enumeration for the fourth million will appear in the volume containing the factor table for that million.

The total numbers of primes of the forms $4n+1$ and $4n+3$ in the first one hundred thousand numbers of each million, taken from the subjoined tables, are

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
0— 100,000	4,784	4,808	−24	9,592
1,000,000—1,100,000	3,642	3,574	+68	7,216
2,000,000—2,100,000	3,463	3,411	+52	6,874
3,000,000—3,100,000	3,368	3,308	+60	6,676
6,000,000—6,100,000	3,193	3,204	−11	6,397
7,000,000—7,100,000	3,182	3,187	− 5	6,369
8,000,000—8,100,000	3,126	3,124	+ 2	6,250

The most noticeable feature in these numbers is the excess of the $4n+1$ primes over the $4n+3$ primes in the groups taken from the second, third, and fourth millions; in these three groups there are 10,473 primes of the form $4n+1$, and 10,293 of the form $4n+3$, the difference being 180, and the total number of primes 20,766.

* "On Long Successions of Composite Numbers," vol. vii, pp. 102–106, 171–176.

† An enumeration of prime-pairs (*i.e.*, of pairs of primes separated by only one number), is given in the "Messenger of Mathematics," vol. viii, pp. 28–33 (1878); and a full account of Burckhardt's and Dase's tables occurs in a paper "On Factor Tables, with an account of the mode of formation of the Factor Table for the Fourth Million" ("Proceedings of the Cambridge Philosophical Society," vol. iii, pp. 99–138, 228–229, 1878). See also "British Association Report," 1878, pp. 172–178.

The results for the whole seven groups are

Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
24,758	24,616	142	49,374

It should be stated that 1 was counted as a prime of the form $4n+1$; 2, of course, was not counted at all.

Tables showing the numbers of primes of the form $4n+1$ and of the form $4n+3$ in each ten thousand of the first hundred thousand numbers of the first, second, third, fourth, seventh, eighth, and ninth millions.

First Million.

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
0— 10,000....	610	619	− 9	1,229
10,000— 20,000....	516	517	− 1	1,033
20,000— 30,000....	486	497	−11	983
30,000— 40,000....	474	484	−10	958
40,000— 50,000....	464	466	− 2	930
50,000— 60,000....	469	455	+14	924
60,000— 70,000....	431	447	−16	878
70,000— 80,000....	454	448	+ 6	902
80,000— 90,000....	445	431	+14	876
90,000— 100,000....	435	444	− 9	879
0— 100,000....	4,784	4,808	−24	9,592

Second Million.

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
1,000,000—1,010,000....	390	363	+27	753
1,010,000—1,020,000....	367	352	+15	719
1,020,000—1,030,000....	369	363	+ 6	732
1,030,000—1,040,000....	347	354	− 7	701
1,040,000—1,050,000....	361	370	− 9	731
1,050,000—1,060,000....	349	349	0	698
1,060,000—1,070,000....	362	354	+ 8	716
1,070,000—1,080,000....	372	350	+22	722
1,080,000—1,090,000....	352	354	− 2	706
1,090,000—1,100,000....	373	365	+ 8	738
1,000,000—1,100,000....	3,642	3,574	+68	7,216

Third Million.

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
2,000,000—2,010,000....	351	354	− 3	705
2,010,000—2,020,000....	345	346	− 1	691
2,020,000—2,030,000....	350	343	+ 7	693
2,030,000—2,040,000....	340	350	−10	690
2,040,000—2,050,000....	343	328	+15	671
2,050,000—2,060,000....	356	340	+16	696
2,060,000—2,070,000....	355	339	+16	694
2,070,000—2,080,000....	343	331	+12	674
2,080,000—2,090,000....	339	347	− 8	686
2,090,000—2,100,000....	341	333	+ 8	674
2,000,000—2,100,000....	3,463	3,411	+52	6,874

Fourth Million.

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
3,000,000—3,010,000....	351	319	+32	670
3,010,000—3,020,000....	325	334	− 9	659
3,020,000—3,030,000....	322	341	−19	663
3,030,000—3,040,000....	322	335	−13	657
3,040,000—3,050,000....	354	317	+37	671
3,050,000—3,060,000....	341	316	+25	657
3,060,000—3,070,000....	334	330	+ 4	664
3,070,000—3,080,000....	357	338	+19	695
3,080,000—3,090,000....	339	347	− 8	686
3,090,000—3,100,000....	323	331	− 8	654
3,000,000—3,100,000....	3,368	3,308	+60	6,676

Seventh Million.

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
6,000,000—6,010,000....	341	320	+21	661
6,010,000—6,020,000....	317	321	− 4	638
6,020,000—6,030,000....	315	349	−34	664
6,030,000—6,040,000....	295	306	−11	601
6,040,000—6,050,000....	322	305	+17	627
6,050,000—6,060,000....	329	337	− 8	666
6,060,000—6,070,000....	322	293	+29	615
6,070,000—6,080,000....	326	327	− 1	653
6,080,000—6,090,000....	311	317	− 6	628
6,090,000—6,100,000....	315	329	−14	644
6,000,000—6,100,000....	3,193	3,204	−11	6,397

Eighth Million.

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
7,000,000—7,010,000....	320	309	+11	629
7,010,000—7,020,000....	306	324	−18	630
7,020,000—7,030,000....	330	324	+ 6	654
7,030,000—7,040,000....	312	336	−24	648
7,040,000—7,050,000....	327	330	− 3	657
7,050,000—7,060,000....	320	308	+12	628
7,060,000—7,070,000....	305	289	+16	594
7,070,000—7,080,000....	333	327	+ 6	660
7,080,000—7,090,000....	324	316	+ 8	640
7,090,000—7,100,000....	305	324	−19	629
7,000,000—7,100,000....	3,182	3,187	− 5	6,369

Ninth Million.

	Number of $4n+1$ primes.	Number of $4n+3$ primes.	Difference.	Total number of primes.
8,000,000—8,010,000....	304	333	−29	637
8,010,000—8,020,000....	295	314	−19	609
8,020,000—8,030,000....	323	308	+15	631
8,030,000—8,040,000....	307	298	+ 9	605
8,040,000—8,050,000....	328	311	+17	639
8,050,000—8,060,000....	331	321	+10	652
8,060,000—8,070,000....	316	312	+ 4	628
8,070,000—8,080,000....	308	315	− 7	623
8,080,000—8,090,000....	323	296	+27	619
8,090,000—8,100,000....	291	316	−25	607
8,000,000—8,100,000....	3,126	3,124	+ 2	6,250

XIV. "On the Effects of Chloroform and Ether on the Radial Tracing." By C. Handfield Jones, M.B. Cantab, F.R.S. Received June 11, 1879.

(Abstract.)

The effects of chloroform and ether on the pulse, when employed to produce anæsthesia in surgical operations, vary considerably. Sometimes the heart's force, estimated by the lowering of the primary rise, with due attention to the spring pressure, is greatly diminished, sometimes it is increased. The rate of the pulse is generally increased. The rhythm is often deranged, sometimes very much, and so-called

respiratory curves appear in the tracing. The tops of the individual beats in the tracing are sometimes flattened, sometimes rendered more pointed; this latter change is scarcely seen when ether is employed. No very marked difference exists between the two agents as regards their effects, but chloroform seems to have the more depressing effect on the heart. The uncertainty of the results from both agents is remarkable.

The opinion is expressed that the curves apparent in many of the tracings are not by any means solely the result of respiratory movements, but more often of deranged cardiac or arterial action. Experimental tracings are given, showing the effects of forced inspiration and expiration, and of the former when the subject had been previously in a state of semi-suffocation, and the blood, therefore, detained in the right cavities of the heart. It is pointed out that the occurrence of waves of contraction in the arteries, such as are seen in the bat's wing, would account for the production of some of the curves, especially those which exhibit the curious phenomenon of diastolic ascent. The view is maintained that the occurrence of curves in the tracing is a morbid sign.

The tendency of narcosis to produce flattening of the tops of the several beats is pointed out, and it is particularly observed that the same change is marked in cases of fatigue of the heart. The heart's force, and the arterial contractility are no doubt weakened in both states—narcosis and fatigue—and this weakening is regarded as accounting, together with the engorgement of the right auricle and veins, for the flattened summit of the beat.

XV. "Preliminary Note on a new Tide-Predicter." By E. Roberts, F.R.A.S. Communicated by G. G. Stokes, M.A., D.C.L., Sec.R.S., Lucasian Professor of Mathematics in the University of Cambridge. Received June 12, 1879.

The Indian Survey Department having undertaken the superintendence of tide-registration around the whole sea-board of India and at the port of Aden, and also the reduction of the observations by the method of harmonic analysis, with the view to the prediction of the tides for the whole of the ports, it became a matter of necessity, in order to save the large outlay which the numerical operation of their prediction would have involved, that an instrument should be constructed to delineate the predictions.

Accordingly, on the recommendation of the Surveyor-General of India, Mr. Roberts was desired to design, and to undertake the construction of, an instrument to include such a number of tide-com-

ponents sufficient to predict the Indian Ocean tides with all the accuracy necessary for practical purposes.

The present machine is the outcome of the recommendation.

The instrument combines the following twenty tide-components :—

The mean lunar semidiurnal;
The first and second overtides of the mean lunar semidiurnal;
Two *elliptic* lunar semidiurnal;
Two *evectional* lunar semidiurnal;
One *variational* lunar semidiurnal;
The mean solar diurnal;
The mean solar semidiurnal;
The lunisolar semidiurnal;
The lunisolar diurnal;
The lunar diurnal;
The solar diurnal;
One lunisolar elliptic diurnal;
One lunar elliptic diurnal;
One compound (Helmholtz) lunisolar semidiurnal;
One compound (Helmholtz) lunisolar quarter-diurnal;
The solar annual; and
The solar semiannual.

Strictly speaking, there is no sensible astronomical tide-component of twenty-four mean solar hours' period, but for the purposes of prediction it is necessary to include such a term, a very regular and sensible result of this period being obtained in the analysis, due probably to wind or temperature. The same remark applies partially to the solar annual and the solar semiannual, the theoretical tides of these periods being very small; the analysed results, however, are of considerable value, being due probably to the effect of rainfall and the regularity of the monsoons. These three components should, therefore, be regarded more as meteorological than astronomical.

The stipulation that the scale for heights should be one inch per foot range for Bombay necessitated a recording barrel of some 18 inches at least; the actual length adopted for the instrument, however, is 22 inches. The delineation of the curves on such a large scale rendered necessary some modification of the system of eccentric pulleys, as fitted on the tide-predicter of the British Association. It was at first contemplated to fit parallel slides only to the larger of the tidal components; the whole of them have, however, been so provided.

The chief difficulty in the construction of the machine is the finding, within reasonable limits, of proportions which shall represent with sufficient accuracy the periods of the several components, in order that the machine may be used for a considerable period of prediction—say, for twelve months. Very great success has been attained in this respect in the present instrument. For instance, the error of the

period of the chief component (the mean lunar semidiurnal) relatively to the mean solar semidiurnal is inappreciable during a whole year's predictions, amounting to about 0°·10 only in a period of fifty years. The largest deviation from strict accuracy is 0°·37, after a run representing twelve months. This is, however, of one of the very small components, and insensible in its results. This part of the design may be therefore regarded as practically perfect.

Each component is provided for setting with a crank, in which a sliding piece is fitted, carrying a steel guiding-pin. The guiding-pin is thrown out by means of a fine-cut screw and micrometer head. An improved parallel slide, carrying a pulley, is also fitted to each component. The guiding-pin works between two parallel adjustable steel jaws at the back of the pulley frame. The pulley frame is fitted with a balance-weight, so that its centre of gravity is in a vertical line through the pulley's axis. The whole slide is counterpoised by a cord and weight, passing over pulleys, in order to relieve the guiding-pin of all strain and to prevent wear. The steel bar of the pulley slide moves freely in two guides drilled out nearly their entire length to reduce the touching parts to a minimum. The other side of the pulley slide is kept in position by a projecting fork or guide, travelling with freedom along a narrow flat brass bar. Both the brass bar guide and the steel rod guide are divided to millimetres; the brass bar for approximate, and the steel rod for the accurate, adjustment of the throw of the crank-pin, for which purpose the upper guide of the steel rod is furnished with a vernier. The milled head of the micrometer-screw is also divided and may be used with the divisions on the brass bar guide. The pulley frame is movable on its steel rod, for the purposes of the perfect adjustment of the pulley about the centre of motion of the axis of the crank.

The axis of the crank carries behind the main plate a fine-toothed wheel, fitted on a slotted cone, with a milled nut for clamping the wheel on its axis. The toothed wheel is driven by an endless screw, carrying a bevelled wheel, which is itself driven by another bevelled wheel on one of the four main axes of the machine. The endless screws and main axes are fitted with counter pivots.

At the back of the machine are fitted the setting dials. Each dial is toothed round its outer edge and movable round its centre by a pinion for adjustment. The axis of the component projects through the setting dial, and carries a steel pointer for setting.

A fine flexible wire fixed to a large screw-head passes alternately under and over the pulleys of the lower and upper series of components, and carries an ink-bottle at its free end. The ink-bottle, fitted with a fine glass point, travels in a geometrical slide, and is suspended to give just sufficient pressure to ensure contact on the paper of the recording barrel.

The recording barrel is fitted with brass pins at equidistant intervals to form the time indications on the paper by perforation. An index for setting is fitted behind the machine at the top of the recording barrel.

The paper, which is continuous and supplied from a reel, passes round two grooved rollers at the back of the main barrel, and is held in position whilst the pins enter the paper, and after receiving the curves is wound round the haul-off drum. The haul-off drum rests on toothed driving-wheels, and by friction turns and slips to accommodate itself at a proper tension to receive the recorded paper. Motion is given to the whole system of wheelwork through the horizontal centre main shaft from a system of clockwork driving-gear at the bottom of the machine, the whole being driven by a weight calculated at about 4 cwt., and controlled by a fan. A warning bell sounds when the weight is nearly run down. The length of the barrel round which the cord is wound is sufficient to give 15,000 turns of the main shaft. This corresponds to about 3 months' run of curves, and will occupy about 1 hour to run off. A year's tide-curves for any port will thus occupy about 4 hours.

The setting of the machine for the prediction of the tide-curves of any part for which the tide-components are known, is as follows :— The dials are first turned so that the epoch or time of maximum is exactly under or above the highest or lowest point according as the component is situated on the upper or lower row of components. The cranks are set vertically (the slotted cone of the wheel on the axis having been first released) and the guide-pin thrown out to its proper range to represent the half-amplitude of the component. The proper positions of the hands having been previously determined by calculation for the time of starting, the hands are set and the slotted cones tightened up. The recording barrel is then set to the time and the wheelwork set in motion. The complete setting occupies only a few minutes.

The large dial in the centre is for showing the progress of the record, which can be marked occasionally to facilitate the entry of the dates on the record after its removal from the machine. A few supplementary pins are inserted in the barrel for the better distinction of the hours. Two speeds of travel can be given to the paper, viz., 1 inch and $\frac{1}{2}$-inch per hour. A fixed rod near the recording pen carries ruling pens for the tracing of base lines, such as dock sills, river bars, or mean tide-levels, or if desired can rule the paper throughout its entire depth to represent feet, metres, &c.

To Sir William Thomson the author's thanks are due for the improved parallel slide and other details, and also to Mr. Légé (the maker of the instrument) for the design of the wheel-gearing.

XVI. "Double Refraction and Dispersion in Iceland Spar. An Experimental Investigation with a Comparison with Huyghens' Construction for the Extraordinary Wave." By R. T. GLAZEBROOK, M.A., Fellow of Trinity College, Cambridge. Communicated by J. CLERK MAXWELL, M.A., F.R.S. Received June 12, 1879.

(Abstract.)

In the "Proceedings of the Royal Society," vol. xxvii, p. 498, the results of some experiments on double refraction in aragonite are given. From them I was led to the conclusion that there were slight differences between Fresnel's theory and experiment, and that possibly there was some connexion between these differences and the wave-length of the light used.

The present paper, of which this is an abstract, is an account of the results of similar experiments on Iceland spar. The method of the experiments is due to Professor Stokes ("Brit. Assoc. Reports," 1862).

A prism is cut from the crystal to be examined, and the position of its faces with reference to the crystallographic axes carefully determined. Then we may show with the usual notation that

$$\tan\frac{\phi'-\psi'}{2}=\tan\frac{i}{2}\cot\frac{\phi+\psi}{2}\tan\frac{\phi-\psi}{2} \quad . \quad . \quad . \quad \text{(I)}.$$

From this by observing the angles of incidence and deviation of a pencil of parallel rays incident on a prism of known refracting angle we can find $\frac{\phi'-\psi'}{2}$, and hence get ϕ' or ψ'; then from the formula

$$\frac{V}{v}=\frac{\sin\phi}{\sin\phi'} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(II)}.$$

V being the velocity of normal propagation in air, v in the crystal, we can find v in certain definite directions. But Huyghens' construction enables us to determine from theory the value of v, and we have hence a method of comparison.

Professor Stokes has made a series of experiments on Iceland spar by this method, and stated his results in "Proceedings of the Royal Society," vol. xx, 443. He found that to the limit of the error of his observations (about 0·0001 in the index) Huyghens' construction was fully confirmed.

The details of his experiments have not been published, and it ap-

peared it might be useful to have a series of results which would serve in future as a test of any proposed theory of double refraction without the necessity of a further recourse to experiment. It is therefore largely with this object in view that I lay the results of my work before the Society.

I have sought to get a series of values of μ or $\frac{1}{v}$ for the three hydrogen lines C, F, g, in directions extending over an arc of 90° from the optic axis.

For this purpose I used four prisms, the edge of each was very approximately perpendicular to the optic axis.

The axis was inclined at angles of −32°, 14°, 38°, and 64° respectively, to the normal to one of the faces of the prism, the angles being considered positive when the optic axis and edge of the prism lie on the same side of the normal. By this means I was enabled to obtain a series of measurements in directions extending from about 5° on one side of the axis to 100° on the other. Each of the measured quantities used finally was the mean of two. The difference between the two was in only 18 per cent. of the observations as great as 20″, so that the difference between the mean value taken and an extreme observation was only rarely as great as 10″.

The spectrometer used was one by Grubb, of Dublin, kindly lent me by Professor Stokes. It reads with verniers to 10″. I found that errors of 10″ in the observed quantities taken so as to produce the greatest effect in the result would produce at most variations of ·00005 in the value of μ.

Precautions were taken to maintain the room at as nearly as possible a uniform temperature.

The position of the faces of the prism with reference to the rhombic faces of the crystal was determined by careful measurement. Each of the results adopted in the calculations was the mean of ten.

The results of the experiments are given in a series of tables.

For the theoretical calculation we have, if μ_1, μ_2, be the principal refractive indices, θ the angle between any wave normal and the optic axis,

$$\frac{1}{\mu^2}=\frac{\cos^2\theta}{\mu_1^2}+\frac{\sin^2\theta}{\mu_2^2}.$$

We require then to find μ_1 and μ_2.

μ_1 is the refractive index for the ordinary ray, and was found from observations on it in different positions of the prisms. μ_2 is the value of μ for a wave normal at right angles to the axis, and was found from observation on light travelling nearly in that direction.

The results are given below.

Values of principal refractive indices in Iceland spar for the three hydrogen lines.

	C.	F.	g.
μ_1	1·65436	1·66779	1·67553
μ_2	1·48456	1·49074	1·49430

The values of θ are determined from the known position of the wave in question, with reference to the normals to the faces of the prism, and of these normals with reference to the optic axis.

The results of the comparison are as follows:—

For the red line C, just in the neighbourhood of the major axis of the elliptic section, the curve given by experiment lies inside that given by theory, the two coincide at about 15° from this axis, henceforth onwards the experimental curve lies outside, and the two coincide again near the minor axis.

The greatest difference amounts to rather over ·0001, about half way between the axes. If we decrease slightly the major axis of the ellipse as given by theory, we may say that the result of experiment is expressed by an oval curve coinciding with Huyghens' ellipse at the extremities of its axes, and lying outside of it for the rest of its course, the greatest difference between the radii vectores being about midway between the axes, and there amounting to about $\frac{1}{10000}$th part of either.

The experiments for the ray F lead to similar results, the differences, however, between theory and experiment being markedly greater, and with the assumed values for μ_1, μ_2, negative near the minor axis, so that the experimental curve is on the inside there.

For g, taking the values of μ_1, μ_2, given above, the differences are negative, and the experimental curve o, the interior, near the axes, positive in the middle part of the arc, and the experimental curve o, lies outside, they amount to rather over ·0002 when a maximum.

Thus, the results for each of the three rays point to the same conclusions, and would lead us to infer that a section through the axis of the surface of wave slowness in Iceland spar would not be an ellipse, but an oval curve, lying slightly outside the ellipse, and that the difference between the two becomes greater as the wave-length decreases.

These differences, however, are very small, amounting to only about ·0002 for the violet ray.

I have, moreover, been able to prove that no variations in the values of the constants of theory or the position of the plane of the prism, with reference to the optic axis, will produce the effect re-

quired to bring theory and experiment into agreement. The results arising from a want of achromatism in the lenses have been considered at some length.

I have also endeavoured to show that, between the limits of the lines C and g, the relation between the refractive index and the wave-length may be expressed by means of the formulæ

$$\mu=\rho+\frac{\rho_1}{\lambda^2}+\frac{\rho_2}{\lambda^4}+\quad . \quad . \quad .$$

Taking the three hydrogen rays, travelling in any the same direction, we have three equations, neglecting $\frac{\rho_2}{\lambda^4}$, &c.,

$$\mu_1=\rho+\frac{\rho_1}{\lambda_1^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

$$\mu_2=\rho+\frac{\rho_1}{\lambda_2^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

$$\mu_3=\rho+\frac{\rho_1}{\lambda_3^3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

The values of ρ found by combining these equations, two and two, differ on the average by about ·00006, and these differences are sometimes positive, sometimes negative.

Taking the mean of the values of ρ and the value of ρ_1 deduced from equation 1, I find the values deduced for μ_2 and μ_3 rarely differ from the observed values by as much as ·00006, the mean only being ·000032. This is considerably less in amount than the differences found by Dr. Hopkinson ("Proceedings of the Royal Society," vol. xxvi, p. 292) between the values of μ, as given by experiment on four kinds of glass, and deduced from three terms of a similar formula,

$$\mu=a+\frac{b}{\lambda^2}+\frac{c}{\lambda^4}.$$

I had hoped that, in the case of Iceland spar, it might be found that the quantity ρ obeyed Huyghens' construction accurately.

This has proved not to be the case; the differences are, on the whole, greater than those for the line g, and of the same sign.

XVII. "On the Vapour Densities of Potassium and Sodium." By Professor JAMES DEWAR, M.A., F.R.S., and ALEXANDER SCOTT, B.A. Received June 12, 1879.

The ingenious method of determining the vapour densities of volatile bodies invented by Professor Meyer* suggested the revision of work done by one of us in connexion with Professor Dittmar,† on the vapour density of potassium. The new method possesses several advantages over that formerly employed, and seemed to be especially fitted to simplify the difficulties of working with substances having the chemical characters of the alkali metals. The fact that a constant temperature need not be maintained unless for a short time, and that all absolute determinations of temperature are unnecessary during the course of the experiments, renders Professor Meyer's system the simplest when the boiling point of the substance is above a white heat.

The apparatus used was simply a reproduction in wrought iron of Professor Meyer's glass one, on a somewhat larger scale. The bottle A had a capacity of 160—180 cub. centims. The tube B was about 2 feet long and in diameter about ·5 inch. A and a considerable part of B was coated with a glaze of borax and then fitted between two crucibles as in figure, and the crucibles filled with fine sand as completely as possible.

This apparatus kept a tolerably constant temperature for a considerable time even when the furnace temperature varied considerably, as it was only slowly affected, owing to the mass of the crucibles and sand surrounding it. The chief difficulty at first was the diffusion of the furnace gases, especially carbonic oxide, through the hot iron. This, however, seems to be completely prevented by the borax glaze. To ensure success, especially in the cases of potassium and sodium, it was found to be essential to clean the apparatus by treatment with dilute hydrochloric acid to remove the larger part of the oxide of iron, then to fuse in it some caustic potash, and thus get rid of the silica, &c., used in the welding in of the tube and bottom of A, after boiling several times with water to dissolve out the potash, it was again treated with dilute hydrochloric acid, and then thoroughly washed and dried. The crucibles were arranged of such a height that the iron was in no place exposed to the gases of the furnace. The fuel used was gas carbon broken into pieces the size of walnuts, this gave a very constant temperature, and the furnace fed with it on more than one occasion fused the bottle A.

* "Deut. Chem. Ges. Ber.," vol. xi, 2253.

† "Proc. Roy. Soc.," vol. xxi.

After the apparatus had been placed in the furnace and had risen to a dull red heat, the tube E was inserted into it and made airtight at C by the india-rubber tubing F, the point of the tube reaching almost to the bottom of A. Hydrogen was now passed into the apparatus by D, and a current kept up until the apparatus was thoroughly reduced; by this time the temperature had risen sufficiently for the experiments



to be proceeded with. The apparatus was now filled with nitrogen, which was likewise passed in at D, the tube E withdrawn, and an india-rubber stopper inserted into C, and the end of D immersed in water or mercury. The weighed substance was then dropped in at C, and the gas expelled, collected in a graduated jar in the usual manner.

The substances whose vapour densities are to be determined, were placed, after weighing, in small iron capsules and kept in them by a plug of wire gauze. This was done in order to ensure their reaching the bottom of the apparatus, and the wire gauze plug also had the advantage of moderating the rate at which the gas came off. The potassium and sodium were placed in tared iron capsules and weighed in dry carbonic acid, and then the plug of wire gauze inserted.

To test the apparatus cadmium was tried and gave very good

results. Zinc was next used, but with very slight success, owing, we believe, to the formation of an alloy of zinc and iron, even although the temperature was far above the boiling point of zinc. This was shown by the wire gauze plug and capsule becoming very brittle, so that the wire gauze might be rubbed to pieces between the fingers. An approach to approximate results was once got with an apparatus which had not been reduced by hydrogen, the coating of oxide of iron and silica having protected the iron from the zinc. Mercury gave good results in this apparatus. With potassium and sodium greater difficulties stood in the way of exact results, as the correct weight of the pieces inserted could not be ascertained, and both metals seemed to have a tendency to behave in the same manner as zinc, viz., to form an alloy with the iron; because the vapour evolved after a few minutes was absorbed completely in spite of all precautions to prevent oxidation. Both of these sources of error tend to make the numbers found for potassium and sodium too high. The mean error due to the weighing of the metals was found by measuring the hydrogen given off by their action on water, and the action of the iron was partially overcome by raising the temperature as much as possible, so as to induce rapid volatilization.

After each experiment the metallic vapours were blown out by a strong current of hydrogen, by inserting (as before in reducing the apparatus) the tube E, and passing the hydrogen in at D, refilling with nitrogen, and proceeding as before.

The following molecular weights are calculated by finding the weight of metal in milligrams required to give 22·34 cub. centims. of vapour at 0° C.+760 millims. pressure.

Cadmium.

0·106	grm.	gave	22·0	c.c. at 0°+760	mm. ∴ mol. wt.	=107·7
0·103	,,		19·4	,, ,,	,,	109·6
0·106	,,		20·8	,, ,,	,,	113·2
0·100	,,		19·2	,, ,,	,,	116·3
0·105	,,		20·4	,, ,,	,,	115·0
0·102	,,		20·7	,, ,,	,,	110·1
0·102	,,		20·9	,, ,,	,,	109·0
0·106	,,		21·1	,, ,,	,,	114·0
						894·9

Mean molecular weight..... 111·86

Mercury.

0·198	grm.	gave	22·8	c.c. at 0°C.+760	mm. ∴ mol. wt.	=194
0·210	,,		23·8	,, ,,	,,	199·6

Potassium.

0·065	grm. gave	18·8	c.c. at 0°C.	+ 760 mm.	∴ mol. wt. =	77·2
0·083	,,	22·4	,,	,,	,,	82·7
0·077	,,	19·6	,,	,,	,,	87·7
0·075	,,	18·5	,,	,,	,,	90·5
0·077	,,	20·0	,,	,,	,,	86·0
0·070	,,	20·0	,,	,,	,,	78·2
0·077	,,	22·6	,,	,,	,,	76.1
0·087	,,	26·1	,,	,,	,,	74·4
						652·8
				Mean molecular weight.....		81·6

Sodium.

0·043	grm. gave	15·1	c.c. at 0°C.	+ 760 mm.	∴ mol. wt. =	63·6
0·067	,,	23·9	,,	,,	,,	62·6
0·047	,,	19·2	,,	,,	,,	54·7
0·040	,,	17·6	,,	,,	,,	50·7
0·059	,,	28·6	,,	,,	,,	46·0
0·040	,,	20·4	,,	,,	,,	43·8
0·045	,,	23·36	,,	,,	,,	43·0
0·037	,,	20·2	,,	,,	,,	40·9
						405·3
				Mean molecular weight.....		50·66

Four pieces of potassium weighed in iron capsules and thrown into water, gave 84·2 as a mean molecular weight, calculated from the hydrogen evolved. Four pieces of sodium similarly treated, gave 49·8 as a mean. The above experiments show that no great accuracy in the determination of the vapour density of sodium and potassium can be attained by this method of working when vessels of wrought iron are employed. The results are, however, conclusive as regards the *normal character of their respective vapour densities.*

XVIII. "The Physical Properties of Liquid Acetylene." By Gerrard Ansdell, F.C.S., Chemical Assistant to the Royal Institution of Great Britain. Communicated by J. Dewar, F.R.S., Professor of Chemistry, Royal Institution. Received June 12, 1879.

The hydrocarbon acetylene, being the only one of its numerous class which can be formed synthetically by the direct union of its

constituent elements, has an especial interest for the chemist. The chief physical constants of this substance are unknown, although its polymerised modification, benzene, has been very thoroughly studied in its physical relations by Regnault. Having one of M. Cailletet's ingenious pumps for the liquefaction of gases at my disposal in the laboratory of the Royal Institution, Professor Dewar suggested a series of accurate determinations of its physical properties in the liquid state, and the present communication deals with the critical point, the tension of the vapour of the fluid at various temperatures, together with the corresponding densities and coefficients of compressibility.

The only notice on the liquefaction of the gas appears to be a short paper by M. Cailletet in the "Compt. Rendus," vol. lxxxv, No. 19, in which he determines the tension of the vapour of the liquid at different temperatures. These tensions, as will be afterwards seen, differ entirely from those obtained in the present paper, one of the reasons appearing to be that instead of using a carefully calibrated air manometer for determining the pressures, he used the ordinary metallic gauge attached to the pump, which is far from being correct.

The pump itself is too well known to need description, suffice it to say, that two of the iron bottles or reservoirs were used, connected with the pump by a piece of fine-bore copper tubing, so as to equalise the pressure, one containing an air manometer registering the pressures from ten atmospheres upwards, and the other the tube filled with acetylene. The two bottles were then placed side by side, and the height of the column of mercury in either read off by means of a cathetometer.

The formulæ used for calibrating the tubes, and also for calculating the volume of the liquefied gas, and the pressure by the air manometer, were those given by Dr. Andrews in his researches on carbonic acid ("Phil. Trans.," 1869 and 1876). The method of preparing the acetylene gas was by the action of alcoholic potash on bibromethylene, the disengaged gas being collected in the form of the red acetylide of copper, by passing it into a strong solution of subchloride of copper in ammonia. This red compound, after being thoroughly washed and boiled with distilled water, was transferred to a flask with dilute hydrochloric acid, the gas driven off by means of a gentle heat, and conducted through a strong solution of caustic soda, to free it from traces of hydrochloric acid, and finally through two small U-tubes with fused chloride of calcium. The perfectly pure and dry acetylene was now passed through the tube to be used for its liquefaction in a slow stream for several hours, and the latter carefully sealed off when all the air had been expelled.

The sealing off requires great care, as unless rapidly done, and the pressure removed from the inside (by cooling the tube) immediately

the point is closed, the acetylene becomes rapidly charred and blows out, a small portion of it being consequently decomposed, and thus interfering materially with the accuracy of the results.

The tube for the tension determinations was of the usual shape used in the Cailletet pump, the internal diameter of the capillary part being about 2·5 mm. This was found to be more convenient than a narrower tube, as a larger reservoir could be used, and consequently a larger quantity of liquid obtained.

The pressure at the different temperatures was always observed when a very slight layer of liquid was formed on the surface of the mercury, as the gas not being entirely free from air, the pressure was slightly increased on filling completely the upper part of the tube.

The following are the tensions obtained compared with those of Cailletet:—

Temp.	Pressure.	Cailletet. Temp.	Cailletet. Pressure.
−23° C.	11·01 atm.		
−10	17·06 „		
0	21·53 „	+1° C.	48 atm.
+ 5·25	25·48 „	2·5	50 „
13·5	32·77 „	10·0	63 „
20·15	39·76 „	18·0	83 „
27·55	48·99 „	25·0	94 „
31·6	56·20 „	31·0	103 „
36	65·36 „		
36·5	65·89 „		
36·9	67·96 „		

The temperatures above zero were kept constant to within $\frac{1}{20}$th of a degree, by allowing a constant stream of water to flow over the tube from a reservoir holding about 10 gallons, in which it had been previously thoroughly mixed. The temperature of −10° was obtained by cooling down alcohol with ice and salt, and that at −23° by surrounding the tube with a narrow glass cylinder containing liquid chloride of methyl, which boils constantly at this temperature; this cylinder being again enclosed in a wider one containing a little phosphoric anhydride to prevent moisture from condensing on the sides.

It was thought interesting to compare the tensions of liquid acetylene with those of the saturated vapour of benzene, being polymeric bodies, although having totally different properties. For this purpose curves were plotted for the two substances, that for the benzene being taken from Regnault's results ("Mem. Acad. Sci., Paris," vol. xxvi, p. 420). They do not, however, run parallel to each other, the benzene having a slower rate of increase at low temperatures, but a quicker rate than the acetylene as the temperature

rises. The curves, however, have no appearance of actually crossing at a higher temperature.

The critical point of acetylene, or that temperature at which no appearance of liquefaction takes place, however great a pressure is exerted on the gas, was found after many careful experiments to be 37°·05 C.

For determining the density and compressibility of the liquid at different temperatures, a tube of much smaller dimensions was used, having a capillary bore of about ·8 mm. in diameter, the whole of the tube having a capacity of 36·3708 cub. centims. This gave a column of liquid about 15 centims. long when the upper part of the tube was entirely full at 15° C.

The density at any particular temperature was taken by forcing the liquid up the capillary tube at that temperature, until the upper part was completely filled; the length of the column of liquid was then read off, its volume calculated, and this observed volume divided into the calculated weight of the gas at zero. They are as follows:—

Temp.		Density.
− 7° C.	.....	·460
− 3	.....	·456
0	.....	·451
+ 4·4	.....	·441
9·0	.....	·432
16·4	.....	·420
20·6	.....	·413
26·25	.....	·404
30·0	.....	·397
34·0	.....	·381
35·8	.....	·364

It has, therefore, about half the density of liquid carbonic acid, and if we take the actual volume of the liquid at −7 as unity, it becomes 1·264 at +35·8, which gives ·00489 as its coefficient of expansion per degree, for the total range of pressure; it is, therefore, only about half as expansible as carbonic acid, whose coefficient is ·010, and is not much more expansible than a gas. Comparing the density of liquid acetylene with that of liquid benzene, the latter is found to be almost exactly three times as great as the former at the same temperature; as for instance, at 0°C. the density of the acetylene is ·456, whereas that of the benzene is ·899; the vapour density, however, of the benzene is three times as great, viz., 2·704.

The apparent compressibility in glass was determined by direct observation, the liquid being forced up in the capillary tube until the latter was completely full, and then the pressure gradually increased,

and the diminution of volume read off at intervals of about 10 atmospheres up to about 180 atmospheres.

Curves were then plotted, showing the volume at different pressures for the same temperature, and from these the coefficient of compression at any temperature and pressure was easily deduced.

The following tables are constructed from the curves.

1.—Mean coefficients of compression of liquid acetylene at different temperatures. Range of pressure from 36·62 to 182·68 atmospheres.

Temp. of acetylene.		Coeff.	Temp. of acetylene.		Coeff.
35° C.	=	·00085	16° C.	=	·00050
28·6	=	·00068	4·4	=	·00038
22·5	=	·00058	0	=	·00025

2.—Coefficients of compression at the same pressure but varying temperatures.

Temp. of acetylene.	Atm. 70.	Atm. 95.	Atm. 120.	Atm. 160.
*49° C.	....	·00343	·00169	·00078
*41	....	·00138	·00099	·00076
35	·00171	·00113	·00078	·00065
28·6	·00122	·00083	·00072	·00050
22·5	·00079	·00065	·00057	·00047
16·0	·00066	·00050	·00049	·00035
4·4	·00047	·00042	·00034	·00032
0	·00041	·00036	·00025	·00029

3.—Coefficients of compression at varying pressures and temperatures corresponding to the same volume.

	Vol. = 97 c.mm.		Vol. = 92 c.mm.	
Temp. of acetylene.	Pressure.	Coeff.	Pressure.	Coeff.
*49° C.	170·8 atm.	·00080		
*41	137 „	·00085		
35	103·2 „	·00093	.... 175·8 atm.	·00065
28·6	70·0 „	·00120	.... 137·8 „	·00063
22·5	....	....	.... 99·2 „	·00065
16·0	....	....	.... 59·5 „	·00066
	Vol. = 101 c.mm.		**Vol. = 89 c.mm.**	
*49	126·3 atm.	·00128		
*41	98·3 „	·00132		
35	72·7 „	·00167		
22·5	....	....	.... 158 atm.	·00054
16·0	....	....	.... 115·6 „	·00056
4·4	....	....	.... 49·7 „	·00058

* These two experiments were, of course, made above the critical point.

It is evident from the above tables that acetylene is governed by the same laws as other compressible liquids, that is to say, its compressibility increases as the temperature rises, but diminishes as the pressure increases. For instance, at a pressure of 95 atmospheres it is three times as compressible at 35° C. as at 0° C.

The volume being the same, the compressibility appears to be nearly the same at different temperatures, which is really due to the curves at high pressures running nearly parallel, thus introducing a corresponding difficulty in the estimation of small differences.

On comparing the compressibility of liquid acetylene with the results obtained by M. Amagat ("Annales de Chem.," 1877) in the case of benzene, it appears to be about seven times as compressible as the latter body, at a temperature of 16° C., and under a pressure of 40 atmospheres. The comparison could not be carried out at higher temperatures, for whereas M. Amagat reaches a temperature of 100° C. with the benzene, I was not able to go beyond 35° C. with the acetylene.

XIX. On the Origin of the Mineral, Structural, and Chemical Characters of Ophites and related Rocks." By Professors W. King, Sc.D., and T. H. Rowney, Ph.D. Communicated by the Treasurer, R.S. Received May 12, 1879.

(Abstract.)

The authors, beginning with (A) "The different kinds of rocks treated of," in their memoir, divide them into two groups. The first, "Silacid Ophites," is represented by serpentinite (common at the Lizard) and other rocks, essentially composed of serpentinous minerals: it includes a subsection, comprising peridolites and some others, all slightly hydrated. The second, "Silocarbacid Ophites," consists of rocks, which, in addition to serpentinous minerals, contain a mineral carbonate—for example, ophi-calcite: its subsection is represented by hemithrenes. The relation of the first group, through its subsection, to ordinary metamorphic rocks, also of the second group, through its subsection, to carrarites and dolomites is pointed out.

As regards (B) "Their mineral character," it is stated that ophites, &c., embrace some fifty or more different minerals, all containing more or less hydrous silicate of magnesia; in addition to which, dry silicates and carbonates are often present. The relation of these minerals to others, essentially anhydrous, as hornblende, diallage, and peridote, is noticed.

Treating on (C) "The structural character of ophites, &c.," the protean nature of their essential mine al, serpentine, is shown by

a description of its fibrous, arborescent, coccolitic, platy, and other allomorphs.

With reference to (D) "The origin of certain mineral, structural, and chemical characters of ophites, &c.," the subject is treated of under different heads:—(1) Fibrous layers in peridote from Elfdalen, in "graphic granite" from Harris, in perthite from Siberia, and in other instances. (2) The alternation of different minerals in laminated ophicalcite and ophi-malacolite has its parallel in other and totally different rocks. (3) The change of the fibres of chrysotile into aciculæ, separated by calcareous interpolations, is illustrated by figures taken from decalcified and polarised specimens of this allomorph from Canada; also from a characteristic specimen of the same from the type-locality, Reichenstein. (4) Branching configurations, such as are assumed by serpentine, are common in hemithrenes from widely different regions: the authors refer to examples, showing that they are residual, resulting from the waste of crystalloids of malacolite. Beautiful examples occur in the calcaire saccharoide (a hemithrene) of St. Philippe, near St. Marie-aux-Mines, in the Vosges, rivalling those in Canadian ophite: and not only are the associated lobulated grains of pyrosclerite covered with a fibrous layer, closely resembling chrysotile in structure; but its fibres are occasionally converted into aciculæ, separated by films of calcite. (5) The presence of calcite under the latter condition, and in connexion with configurations of serpentine and malacolite, as well as lobulated grains of these and other minerals, the authors ascribe to chemical changes similar to pseudomorphism among minerals. (6) It is contended that no minerals are incapable of resisting changes of the kind, even those regarded as the most insoluble. The experiments of Bischof, the Professors W. B. and R. E. Rogers and others, show that hypersthene, enstatite, serpentine, and various mineral silicates, digested in water containing carbonic acid, are convertible into carbonates. (7) Cases are mentioned of rocks, essentially composed of mineral silicates, which have thus become changed; as diorite from Jersey, granite and a porphyritic feldsite from near Galway, which have had certain of their mineral silicates replaced by serpentine and calcite.

The latter cases bring on a chapter (E) "On rock-metamorphism generally." The authors divide metamorphic rocks into two groups—mineralised, and methylosed; the former consisting of members which have had their original sedimentary components mineralised into gneiss, hornblende-schist, &c.; and the latter of members, thus mineralised, but which, through the intervention of chemical reactions, have been converted into ophites, &c.: methylosis is to rocks the same as pseudomorphism to minerals. After briefly referring to the theory promulgated by Leibnitz in his "Protogæa," which anticipated many points now generally held as to the origin of the metamorphics, they

examine the doctrine advocated by Sterry Hunt; and contend that it is altogether untenable, both from his own arguments, and a body of unquestionable counter-evidence. Repudiating a doctrine which regards the rocks in question as still being in their original or quasi-original condition, formed at the bottom of a primæval ocean, through the chemical precipitation of substances which it held in solution, the authors express themselves in accordance with the prevailing opinion that they were originally ordinary argillaceous, arenaceous, and other sediments, which, through being buried at great depths, have undergone various changes—some ending in their mineralisation and others in their methylosis. Sterry Hunt's doctrine is further contested by evidences adduced of regional metamorphism pertaining to various post-Archæan periods, whose crystalline or mineral effects are identical with those which he restricts to pre-Cambrian ages, and which he presumes to have been produced by chemical precipitations from seas of the time.

The mineralised metamorphics having thus far principally engaged their attention, the authors next touch upon the (F) "Methylosed metamorphics—ophites." Taking, as their standpoint, the carefully worked out conclusion of Blüm, Bischof, Rose, and others, that serpentine, as a mineral, is in all cases the product of pseudomorphism, it is contended that rocks essentially made up of it, adding other secondary minerals in certain kinds, have necessarily undergone chemical changes. Cases are cited, such as the serpentinite of the Lizard, which they were the first to show, from its containing pseudomorph crystals after augite, had been originally a porphyritic dolerite. One of the Cannover Isles, in Lough Corrib, contains a mass of serpentine, which is shown to be a methylosed diorite or tremolitic rock.

The evidences offered by Bischof, Heddle, and other writers, as to the conversion of serpentinous and other siliceous rocks into calcareous masses are adduced by the authors in confirmation of their view respecting (G) "The methylotic origin of hemithrenes, &c." Additional original evidences are brought forward with the same purport. A volcanic or doleritic dyke intersects gneiss on Mr. Frederick Twining's estate, adjacent to Cleggan Bay, Connemara; where, not only is the gneiss converted into hemithrene, consisting of malacolite, peridote, serpentine, calcite, and other minerals, but the dyke itself is charged with calcitic matter. Another case occurs at St. Philippe, Vosges, where gneiss incloses dyke-like masses of hemithrene, as to conclusively prove, in the opinion of the authors, that the latter are chemically changed products of the former, effected by permeating streams of heated water containing a carbonate in solution. The labours of Delesse have shown that the region around abounds with masses of the kind.

The rocks described having undergone such remarkable changes,

the authors have been induced to make investigations as to (H) "The origin of the minerals characteristic of ophites, &c., especially peridote." With certain exceptions the minerals referred to are considered to be of secondary origin, the exceptions being those remaining unaffected by secondary agencies. Serpentines, malacolite, phlogopite, chlorite, enstatite, and a number of others are all considered as secondary minerals. Peridote, notwithstanding that it is generally considered to be an original mineral in the same sense as the hornblende, feldspar, mica, &c., of granite and other plutonic rocks, is regarded by the authors as a product of alteration in all its relations, and circumstances of occurrence. Its presence in granites, basalts, and lavas has given rise to the belief that it is of igneous origin: nevertheless its occurrence in mineralised and methylosed rocks (gneiss, and ophite of the sedimentary section) is held as proving the contrary: and the authors feel themselves justified in assuming that it is as much a secondary product as the zeolites and pseudomorphs found in granites, basalts, and lavas. Many of the crystals occurring in basalts and lavas, which have been taken for peridote, are in their opinion pseudomorphs after augite and hornblende.

Repudiating the doctrine that the Archæan rocks are the result of chemical precipitations, and entertaining the strongest doubts that life has been to any extent concerned in their formation, the authors, in a chapter (I) "On the origin of the Archæan crystalline limestones of Canada," apply their views on hemithrenes to the present subject; and they arrive at the conclusion, from various considerations, that the rocks in question are methylosed products; but which, before this change took place, existed as gneisses, hornblende-schists, and other mineralised silacid metamorphics.

The question (J) "Why limestones are so rare in formations immediately succeeding the Archæans" is discussed in connexion with the facts that calcareous organisms are rare or not present in the formations referred to, and that calcareous rocks are abundant in the preceding systems—the Archæans. These facts are held to be in unison with the authors' conclusions stated in the last chapter, and to favour the view that the Archæan limestones with their present constitution were not available as materials for the production of calcareous rocks in the earliest Cambrian age.

(K.) "The genetic difference between mineralised and methylosed metamorphism" is explained by assuming that water has been an important factor in both cases; but as the minerals in the first group are for the most part anhydrous or dry species, it is assumed that the original (? hygroscopic) water, which its members contained in their condition as sediment, was sufficient for their mineralisation; on the other hand, as the minerals composing the members of the second group are chiefly hydrous, it is contended that their methylosis has

been effected by additional water penetrating them, and flowing from extraneous or foreign sources. Compared with each other, mineralised rocks may be classed as xerothermal, and methylosed as hydrothermal.

Various evidences are adduced to show that (L) "Some ophites have been originally igneous, and others sedimentary rocks,"—a conclusion favouring their secondary, and consequently their methylotic origin.

(M.) "Some crystalline limestones are simply mineralised," such as carrarite; though rocks closely related to them,—viz., "Dolomites, have undergone methylosis." With regard to the latter, however, the authors do not accept Von Buch's theory of dolomitisation in its general application. Admitting various kinds of this phenomenon, they conceive the change in certain well-known cases has been effected by the action of the magnesian constituents of sea water on subjacent beds of limestone; for example, during the closing portion of the Triassic period, as strongly supported by geological evidences, determined by Ramsay and others, the seas in certain European regions became dried up or reduced; and their water, loaded with magnesian salts, sank through the subjacent sandstones and marls into the Permian limestones, thus converting them into dolomites. Irish corroborative cases are mentioned. The dolomites of the Tyrol are held to have originated in the same way; but it is admitted to be probable that the predazzite of the Canzacola mountain, Val di Fassa, was dolomite that became hydrated by the heated water which accompanied the eruption of the immediately adjacent and overlying monzonite. "Serpentinisation effected in deposits without the intervention of mineralisation" is admitted in the production of the magneso-argillite at Vallecas near Madrid, also of that in the Paris Basin, and other localities; for Sullivan and O'Reilly have shown that it was originally a non-magnesian deposit.

The authors conclude by treating of (N) "The Chrono-geological range of Ophites, &c., and the age of their methylosis." Offering merely possible suggestions as to the age in which this phenomenon took place in what may be regarded as the oldest ophites (as the subject is beset with considerable difficulties), instances referable to secondary periods, as the dolomites and serpentine rocks of the Tyrol, &c., are briefly noticed; but they refer more confidently to the methylosed euphotides, &c., of Northern and Central Italy, which, having burst through Cretaceous limestone (alberese), Eocene sandstones and schists, have incontestably produced gabbro verde during late Tertiary ages. Moreover, it would appear, from the discoveries of Achiardi, that argillaceous schists, in Tuscany, are now being serpentinised by the action of magnesiated water. And, taking the wide range of evidences, which have been adduced, into consideration, it can scarcely be doubted that the same process is still in operation in deep-seated rocks, permeated by heated waters.

XX. "Note upon the Behaviour of Oxalic Acid and the Oxalates of the Alkalies, and of Potassium Iodide, in Sunlight."

By Arthur Downes, M.D., and Thomas P. Blunt, M.A. Oxon. Communicated by Professor Huxley, Sec. R.S.

Received June 16, 1879.

In a previous communication* we stated that a decinormal solution of oxalic acid may be entirely destroyed by sunlight, the decomposition being due to oxidation. By a comparison, under like conditions, of oxalic acid and neutral potassium oxalate respectively we further concluded, since the last-named salt appeared to remain unchanged while the former was wholly decomposed, that this destruction of oxalic acid by light was due to the oxidation of its hydrogen.

During the present year, however, we have made a closer and fuller investigation, which shows that the immunity of neutral potassium oxalate is not absolute, and that we must further extend our views as regards the power of light to promote the oxidation of certain of the elements.

A measured quantity of normal solution of oxalic acid was carefully neutralised with—

1. Potassium hydrate.
2. Sodium „
3. Ammonium „
4. Lithium carbonate.

The solution was then diluted up to ten times the volume of the normal oxalic acid originally taken, so that in each case the resultant neutral solutions contained the same weight of the oxalic radical, that, namely, which would be found in an equal bulk of decinormal oxalic acid. Selecting sixteen test-tubes of similar size and bore, we divided them into four sets of two pairs each. In each set respectively we placed one of the above-named solutions (each tube receiving 50 grain measures carefully measured by means of a pipette), a corresponding batch being charged in precisely the same way with a freshly prepared decinormal solution of oxalic acid. All were next raised to boiling point, to prevent development of organisms, and plugged with cotton wool. They were then placed on an outside window-sill, one pair of each batch being incased in laminated lead, the other left freely exposed to the sun's rays. This was on March 28. On each of the dates given in the subjoined table an insolated and an incased tube of each set were compared by titration in the following manner. By appropriate dilution and measurement exactly a fifth part of the con-

* "Proc. Roy. Soc.," vol. xxviii, p. 204.

tents of each tube was taken, treated with an excess of sulphuric acid, and a standardised solution of potassium permanganate run in until a faint permanent pink tinge remained in the solution when warmed. A direct measure, inversely as the quantity of permanganate required, was thus obtained of previous oxidation.

Experiment started March 28.

Potassium permanganate solution required for oxidation.*				
	April 25th.		June 9th.	
	Incased.	Insolated.	Incased.	Insolated.
Oxalic acid.....	7·5 c.c.	2·0 c.c.	7·25 c.c.	0·0 c.c.
Ammonium oxalate.....	7·5 „	5·1 „	7·3 „	2·2 „
Sodium oxalate	7·4 „	6·05 „	7·2 „	5·2 „
Potassium oxalate	7·5 „	6·1 „	7·1 „	5·4 „
Lithium oxalate.....	7·3 „	6·2 „	7·25 „	6·0 „
Freshly prepared decinormal solution of oxalic acid.....	7·3 c.c.		7·2 c.c.	

Our previous statement as to the total oxidation of oxalic acid in sunlight is thus confirmed, but we learn for the first time that in no case does an oxalate of an alkali entirely escape the oxidising action of sunlight, although whether the various alkali salts always obey the relative proportions of the table above given is a question on which at present we express no opinion whatever. It is, however, an interesting question which constituent of their molecules is the subject of oxidation. Hydrogen, except in the case of the ammonium salt,† is out of question. The uniformity of the initial action indicated the possibility of an oxidation of carbon, but a collateral study of the

* Disregarding the trifling differences in the decimals as due to unavoidable error the incased liquids remain practically unchanged. From other experiments, not here given, however, it would appear that to obtain this stability the solutions must be boiled, probably on account of the tendency to development of low organisms in the dark.

† Speculation as to the cause of the greater oxidation of this compound, as compared with the other alkali-oxalates in the above experiment, would be premature; and, although the suggestion that a portion of the hydrogen of the NH_4 is seized upon, offers itself, it is only right to say that the possibility of other explanation has presented itself to us. This we leave for the present.

We may remark incidentally, that a mixture of gaseous ammonia (NH_3) and oxygen in a glass tube over mercury, underwent no change in volume after months of insolation last year.

well-known decomposition of potassium iodide in sunlight has led us to believe that in this case (KI) the phenomenon is of similar character. The nature of this decomposition of potassium iodide has been much discussed, and observers, misled as it seems to us by a supposed analogy with ordinary photographic reactions, have singularly failed to recognise the possibility of the oxidation of metallic elements by light. Hence they have ascribed this behaviour of potassium iodide to acids, carbonic and other, of the atmosphere. On this point, however, we have satisfied ourselves; first, that in a Sprengel vacuum a 10 per cent. solution remains perfectly colourless in sunlight; and, secondly, that decomposition invariably occurs in an insolated solution when no other gases than nitrogen and oxygen are present.* Here the oxidation can only be that of the alkali metal, and analogy would suggest a similar inference in the case of the oxalates. We are, however, still engaged on the investigation, and our present object is rather to qualify the statement in our previous paper as to the supposed indestructibility of an alkali-oxalate by sunlight, than to advance to a conclusion for which the evidence is not yet sufficient.

XXI. "Preliminary Experiments on the Effects of Long-continued Stress on the Elasticity of Metals." By J. T. Bottomley, M.A., F.R.S.E. Communicated by Sir W. Thomson, F.R.S. Received June 14, 1879.

The following paper is intended to describe the results of experiments on elasticity of wires which have been carried on during the past two years, in the Natural Philosophy Laboratory in the University of Glasgow.

In 1875, at the Glasgow meeting of the British Association, a Committee was appointed, at the suggestion of Sir William Thomson, for the purpose of commencing secular experiments on the elasticity of wires, and a grant of money was placed at the disposal of the Committee.

These experiments have accordingly been commenced. An iron tube about 70 feet long has been erected in the tower of the University. Two wires of gold, two of platinum, and two of palladium,

* The mode in which we insured, as we believe, the absolute freedom of the air employed from acid impurity was the following:—Exhausting tubes to a good Sprengel vacuum, we admitted—by means of the apparatus we described in "Proc. Roy. Soc.," vol. xxviii, p. 210—air which had been standing, with frequent agitation, for six days over a solution of potassium hydrate. To make doubly sure, the exhaustion was again repeated and the purified air a second time admitted. The tubes thus treated acquired a brown tinge in light as quickly as did similar ones simply plugged with cotton wool.

have been hung up, one of each pair with a light stretching weight, and the other with a weight equal to half its breaking weight. Marks have been put upon the wires; and a cathometer has been erected opposite to a window in the tube for the purpose of observing the difference of elongations in each pair of wires.

In connexion with the commencement of these experiments we gave as an experimental exercise to some of the students in the Natural Philosophy Laboratory, the comparison of the stretching and breaking of wires at different speeds. Interesting results followed, and the investigation was then taken up systematically, and is still being carried on.

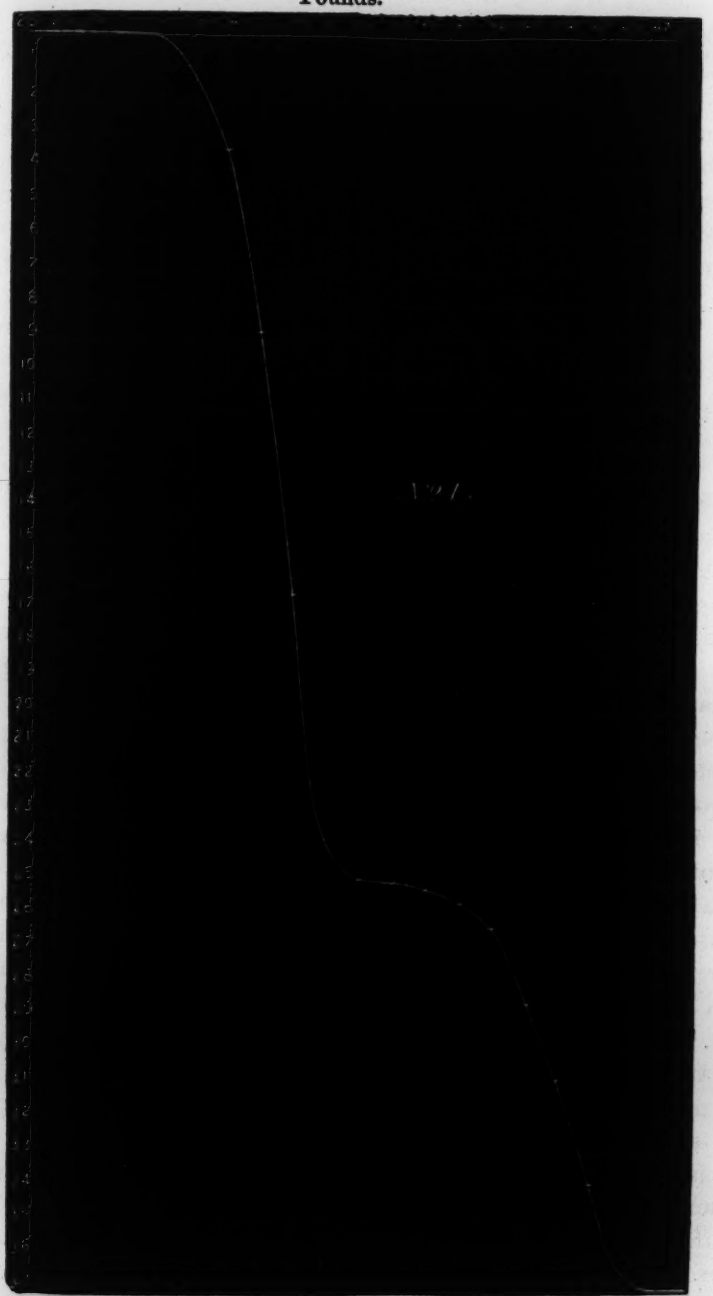
Pure soft iron wires were obtained, and were hung up with an arrangement for measuring the amount of elongation. They were then loaded more or less quickly till they broke, and the elongation with different loads was determined. A large number of curves, of which a specimen is given below, have been traced showing the elongation produced by different loads under given circumstances. They will be considered in detail in a future communication.

The following is a general account of the experiments that have been made.

Lengths from the same hank of the wire, which was specially drawn for the purpose, were hung up and a scale pan having been attached to the lower end, weight was put on till the scale pan and added weight together made up 28 lbs. This was the greatest weight that could be put upon the wire in its natural condition, without causing *permanent* elongation. The wire was generally left with this load applied to it for 24 hours. Sometimes the wire ran down very slightly in the 24 hours with the load, but not often.

Some of the wires were then broken quickly. One pound was added to the scale pan, say every three or every five minutes, except in cases where the wire was visibly running down, when no fresh weight was added till the running down stopped. Another set of wires was broken by adding a pound every 24 hours; another set was broken by adding three-fourths of a pound every 24 hours; another set by adding half a pound every 24 hours; and an experiment was commenced in which a quarter of a pound was to be added each day, but this was discontinued, and to this wire there was added one-sixteenth of a pound, not at regular intervals, but only after the wire had borne the weight on it for upwards of 24 hours without suffering any perceptible elongation. When a small weight is added to a wire, as was done in this case, the elongation takes place very slowly. The wire goes on running down slowly, often for several days, and it was in order to cause the whole effect of the previous weight to have been produced before any fresh weight was added that this mode of adding weight was adopted. The experiment unfortunately was brought to

an end after about five months had been expended on it by the wire becoming rusted through. A fresh wire was, however, suspended some months ago, and was carefully varnished to prevent rusting. It is now under experiment.



Wire No. 1_2, put up February 18, 1878, broke March 10 with 48 lbs., elongation 37 cent., or 8·132 per cent.

The following tables contain a considerable part of the results of these experiments.

Two kinds of wire were used both of pure soft iron wire, drawn for the purpose of these experiments, but differing slightly in the treatment during the annealing process. They are distinguished in our note books as "Black annealed wire" and "Bright annealed wire." Tables I and III show the results of quick breaking of these two wires, and Tables II and IV the results of slow breaking. It will be seen that the slow and long-continued application of stress has invariably strengthened the wire, a result in itself of great importance. It should tend to correct some of the views commonly held as to the danger of applying severe loads, as tests to iron structures, such as steam boilers. It appears that the iron wire here employed was in more than one case actually improved in strength by about 10 per cent. As the wires tested were about 16 feet long and very uniform, it is very unlikely that the wires 8, 9, 14, 15, in Table II, were abnormally strong over such a length; while the wire 12, in Table II, the only one which is not strengthened, may readily have contained some slight flaw, or may have been broken by some jar in putting on the weights.

The result as to amount of elongation is no less important. Wires broken rapidly receive an elongation of over 25 per cent. on the average; while the same wire broken slowly is only elongated by about 7 per cent., and in the case of the bright annealed wires so small elongation as 4·79 per cent. was found with a breaking weight increased by $5\frac{1}{2}$ per cent.

A specimen curve showing the relation between stretching weight and elongation is here given. It will be seen that the stretching does not go on uniformly. At the beginning the wire resists stretching very much, till the weight of 32 lbs. is reached. If, however, a weight of 29 lbs. be put on at first this resistance is overcome, and the curve goes uniformly down till a weight of about 35 lbs. is reached. About this point the wire appears to become abnormally stiff again, and resists weights up to about 42 lbs. This stiffness is again broken down by this weight and the wire runs rapidly down with increasing loads. These features are noticed in every curve and at very nearly the same loads.

It might be supposed that the stiffness at the very beginning is due to a skin of oxide acquired in the drawing and annealing process. This is not the case however. The wire may be cautiously elongated in such a way that the skin must be broken through *and the stiffness is still retained.* The stiffness acquired after loading to the extent of 35 lbs. could not in any case be accounted for by skin-stiffness.

Table I.—Quick Breaking of Iron Wire, ·036 inch in diameter, at various speeds. "Black Annealed Wire."

Number of experiment.	Rate of adding weight.	Breaking weight in pounds.	Per cent. of elongation on original length.
1	½ lb. per minute.	45	25·4
2	1 „ 5 minutes.	45¼	25·9
3	„ „ 5 „	45¼	24·9
4	„ „ 4 „	44¼	24·6
5	„ „ 3 „	44¼	24·9
6	„ „ 3 „	45¼	29·6
7	„ „ 5 „	44¼	27·8

Table II.—Slow Breaking of Iron Wire, ·036 inch in diameter, at various speeds. "Black Annealed Wire."

Number of experiment.	Rate of adding weight.	Breaking weight in pounds.	Per cent. of elongation on original length.
1	1 lb. per 24 hours	48	7·58
2	„	48	8·13
3	„	47	7·05
4	„	47	6·51
5	„	47	8·62
6	„	47	5·17
7	„	46	5·50
8	¾ lb. per 24 hours	49	8·50
9	„	48¼	8·81
10	„	46	7·55
11	„	46	6·41
12	„	45½	6·62
13	½ lb. per 24 hours.	48	8·26
*14	„	50	8·42
15	„	49	7·18

* Towards the end of the experiment weights were not put on at regular intervals, but much slower than ½ lb. per day.

Table III.—Quick Breaking of Iron Wire, ·036 inch in diameter, at various speeds. "Bright Annealed Wire."

Number of experiment.	Rate of adding weight.	Breaking weight in pounds.	Per cent. of elongation on original length.
1	1 lb. per 5 minutes	44¼	28·5
2	„ 5 „	44¼	27·0
3	„ 4 „	44¼	27·1

Table IV.—Slow Breaking of Iron Wire, ·036 inch in diameter, at various speeds. "Bright Annealed Wire."

Number of experiment.	Rate of adding weight.	Breaking weight in pounds.	Per cent. of elongation on original length.
1	1 lb. per 24 hours	47	6·92
2	½ " "	47	4·79
3	½ " "	46½	6·00

I have to acknowledge the very great assistance rendered to me in the carrying out of these experiments by Mr. Sinclair Couper, who has not only most faithfully carried out the work entrusted to his charge, but has in many cases originated and carried out experiments at various points of the inquiry.

XXII. "Note on the paper (read April 24)* 'A Summary of an Inquiry into the Function of Respiration at Various Altitudes on the Island and Peak of Teneriffe.'" By William Marcet, M.D., F.R.S. Received June 16, 1879.

In the above-stated communication, after alluding to Dr. Rattray's observations† and remarking that I had formerly held the same views as he did, namely, that less carbonic acid was exhaled under increasing temperatures of the sun; I add, I am now compelled, however, to alter this view, and to conclude that more carbonic acid is formed in the body under a tropical or nearly tropical sun than under temperate latitudes.

Lest I should be considered as ascribing to the sun's heat a power which so far has not been acknowledged by physiologists, namely, that of increasing the formation of carbonic acid or the combustion in the body, I must beg to explain in a few words how the question now stands. I shall end by asking leave to suspend any opinion as to the cause of the increase of combustion in the body I observed to take place on the Island of Teneriffe, until more direct experiment has been brought to bear on the subject.

From trustworthy experiments by known authors, heat and light are found to act differently with respect to the formation of carbonic acid in the animal and human body; increased heat, as a rule, diminishes the production of this gas, and it is generated in excess under increased light.

* "Proc. Roy. Soc.," vol. xxviii, p. 498.

† "Proc. Roy. Soc.," 1870, 1871, 1873.

The experiments of Edward Smith,* show that more carbonic acid is expired by man in summer than in winter. Vierordt† finds that a change of temperature, equal to 10° F., alters the amount of carbonic acid expired by 0·0183 per cent., increasing it to that amount by a fall, and raising it by that figure through an increase. These observations were made under temperatures ranging between 38°·7 and 75°·7 F.

Letellier‡ in 1845, investigated the action of low and high temperatures on the production of carbonic acid in the animal body. Animals were made to breathe under a glass receiver during periods varying from half an hour to several hours. The results are stated in tables which show generally that at the temperature of 32° F., the carbon burnt is twice as great as at 86° F., while at the ordinary temperature intermediate figures are obtained, there being at times an increase and at others a falling off in the amount of carbonic acid formed. This applied equally to a canary bird, a dove, two mice, and a guinea-pig; there was, however, a peculiarity with respect to two of the guinea-pigs. Out of three experimented upon, No. 1 yielded at high and low temperatures 1·457 CO_2 and 3·006 CO_2 respectively, or nearly in the proportion of 1 to 2, while with guinea-pigs Nos. 2 and 3 the proportion of carbonic acid expired was 1 to 1·5 or 1·4, instead of 1 to 2. In these cases, however, both animals had been clearly very uncomfortable under the high temperature, and one died. The survivor's temperature had risen by 3° and that of the other by 5°·5. But the circumstance perhaps most worthy of notice, as Letellier expresses it, is that heat which the author finds under ordinary circumstances to reduce the formation of carbonic acid in the body, no longer exerted the same influence at night on a dove submitted to experiment. That bird at the ordinary night temperature would have given 0·264 grm. CO_2, but on raising the temperature to between 86° F. and 104° F., it could not be made to exhale less than 0·284 grm. This observation is very curious, especially considering that light, according to Moleschott, shows a marked influence in promoting the formation of carbonic acid and darkness in checking it. I do not find that this experiment was repeated, still, if even the only one of its kind, the result is remarkable.

We now come to the interesting experiments of Moleschott§ made in 1855, on the influence of light on the production of carbonic acid in animals. Frogs were submitted to experiment; he found that 100

* "Phil. Trans.," 1859.

† "Physiologie des Athenens," 1845, and "Brit. and Foreign Med. Review," 1846.

‡ Letellier, "Annales de Chimie et de Physique," Avril, 1845.

§ T. Moleschott, "Wittelhöfer Wiener Medicinische Wochenschrift," 1855, and "Comptes Rendus de l'Acad.," t. 41, 1855.

grms. of these animals emitted in 24 hours in the light 654 mgrms. carbonic acid, and in darkness 522, giving the proportion of 1 to 1·25, the increase of temperature accompanying the light's influence only amounted to 2°·93 C., so that it could not be considered as having exerted any influence. In another paper (same vol. "Compt. Rend.") Moleschott determines the degree of the light used in his experiments by means of photographic paper exposed for five minutes to the light, and compares the depth of tint obtained with standard colours. He finds that the carbonic acid produced under a feeble degree of light is to that exhaled under a very powerful light as 1 : 1·18, the increase of temperature being only by 1°·65 C. He concludes:—1. That frogs for similar weights, and in equal periods, exhale from one-twelfth to one-quarter more carbonic acid when breathing under the influence of light than while in the dark, so long as the temperatures are equal or vary but slightly. 2. The production of carbonic acid increases in a direct ratio with an increase of the light to which the animals are submitted. 3. The influence light exercises towards increasing the amount of carbonic acid emitted, acts partly through the eyes, partly through the skin.

Our knowledge on the influence of light upon the growth of animals, has been extended quite lately by the interesting experiments of Emile Young, of Geneva.* The favourable influence of violet and blue light on the nutrition of animal tissues is clearly demonstrated in that paper. He cites M. Béclard who, in 1858, observed that the worms from flies' eggs acquired growth in violet and blue light much more rapidly than in white and green light. Mr. Emile Young, by experimenting on frogs' eggs, observes that tadpoles become developed much more speedily in violet and blue light than in red and green light, and, moreover, that when grown in white light, and then exposed without receiving any food to the influence of coloured lights, they die soonest under the violet rays. He also states he has seen that tadpoles reared under violet light resist starvation longest if placed afterwards under white light. He concludes that violet light apparently promoted the nutrition of tadpoles more than any other coloured light. An allusion is made in his paper to the experiments of General Pleasanton, who found, by keeping young sows under violet and white glass during six months, that those under the violet glass gained, for equal weights, 34 lbs. more than those under white glass. Experiments on oxen by the same author yielded similar results.

From this mass of experimental work bearing on the influence of heat and light on animal combustion, it may be concluded:—1. That an increase of temperature causes, or strongly predisposes to a

* Emile Young, "Influence des Lumières Colorées sur le Développement des Animaux." "Archives des Sciences Physiques et Naturelles," Mars, 1879.

falling off in the production of carbonic acid, or lessens combustion in the animal body. 2. That increase of light, especially of violet and blue light (the most actinic rays), either causes, or greatly predisposes to the formation of an increase of carbonic acid, thereby promoting animal combustion.

These results do not, it will be observed, apply to the united action of heat and light, but to the influence of heat and light considered separately. They are, I fear, calculated to give us but little assistance towards a knowledge of the influence of tropical climates on the phenomena of combustion in progress in the healthy human body.

The only direct experiments and observations on that subject, with which I am acquainted, are those of Dr. Rattray, alluded to in my paper. He finds that the body loses weight in passing from a cold into a tropical climate, and regains it on its returning under a colder latitude; and, moreover, that in tropical countries the temperature of the body has a distinct tendency to rise slightly above its normal condition in temperate zones. The increased waste of tissue, and the rise of temperature of the body under a tropical sun, even should it only be trifling, appear to me to show a tendency to increased combustion in the body in a tropical climate; and the marked excess of carbonic acid I found to be exhaled under the sun of Teneriffe in July and August, over that given out near the Lake of Geneva and in the Alps, is quite in keeping with Dr. Rattray's observations.

What is the cause of this increased combustion and waste of tissue under a tropical climate? Is it the excess of the sun's heat, is it the increase of the sun's light, or is it a combined action of both? After what has been reported in this note, it must be concluded that the answer is difficult to give, and we must await further experiment to be able to explain this very interesting and important phenomenon.

So far my two communications to the Royal Society, on the influence of altitude upon respiration, have been strictly limited to brief summaries. I have not yet given the history of what has been done on that subject, otherwise I would have had many important physiological contributions to cite; these I shall propose to consider on another occasion.

XXIII. "Agricultural, Botanical, and Chemical Results of Experiments on the Mixed Herbage of Permanent Meadow, conducted for more than twenty years in succession on the same land." By J. B. Lawes, LL.D., F.R.S., F.C.S., and J. H. Gilbert, Ph.D., F.R.S., F.C.S., F.L.S. Received June 16, 1879.

(Abstract.)

In the experiments at Rothamsted with different manures, wheat has now been grown for thirty-six years in succession on the same land, barley for twenty-eight years, and oats for nine years. Somewhat in like manner, but with some breaks, beans have been grown over a period of more than thirty years, clover for many years, and "root-crops" (turnips, sugarbeet, or mangel-wurzel) also for more than thirty years. Each of these individual crops has exhibited certain distinctive characters under this unusual treatment. But, withal, those of the same natural family, wheat, barley, and oats, for example, have shown certain characters in common, those of the Leguminous family characters widely different, whilst the so-called root-crops, belonging to the Cruciferous and Chenopodiaceous families, have exhibited characteristics differing from those of either the Gramineæ or the Leguminosæ.

Compared with the conditions of growth of any one of these individual crops grown separately, those of the mixed herbage of grass land are obviously extremely complicated. Thus, it comprises, besides numerous genera and species of the gramineous and leguminous families, representatives also of many other natural orders, and of some of great prominence and importance as regards their prevalence and distribution in vegetation generally. And if, under the influence of characteristically different manuring agents, as has been the case, there have been observed notable differences in the degree of luxuriance of growth, and in the character of development, even between closely allied plants when each is grown separately, and much greater differences between the representatives of different families when so separately grown, might we not expect very remarkable variations of result, when different manures are applied to an already established mixed herbage of perhaps some fifty species growing together, representing nearly as many genera and more than twenty natural orders?

Such, far beyond what could have been anticipated, has been the case in the experiments described. So complicated, indeed, have been the manifestations of the "struggle" that has been set up, that, even after more than twenty years of laborious experiment, both in the field and in the laboratory, and following up both the botany and the chemistry of the subject, we can hardly claim to have yet done

much more than reach the threshold of a very comprehensive inquiry. Still, we hope to establish some points of general interest; and possibly to indicate promising paths of future research.

From the title of our paper, it will at once be concluded that the experiments were originally undertaken and arranged from an agricultural point of view. But, as experimenting on the feeding of animals soon led us into lines of inquiry of even more interest to the chemist, the animal physiologist, and the dietetician, than to the agriculturist, so the investigation of the effects of different manures on the mixed herbage of grass land has led us far beyond the limits of a purely agricultural problem, and has afforded results of more interest to the botanist, the vegetable physiologist, and the chemist, than to the farmer. Indeed, agriculture, the most primitive, and commonly esteemed the rudest of the arts, requires for the elucidation of the principles involved in its various practices, a very wide range of scientific inquiry; and the investigation of them may, in its turn, contribute facts of interest to the student of various and very distinct branches of natural knowledge.

It will be readily understood that, as a necessary foundation for the discussion of the botany and the chemistry of the subject, it will be essential first to put on record, and call attention to, what may be distinguished as the agricultural data. It is proposed, then, to arrange and consider the results obtained under the following heads:—

Part I. ..... The Agricultural Results.
Part II. ..... The Botanical Results.
Part III. ..... The Chemical Results.

The general scope, objects, and results of the whole inquiry may be briefly indicated as follows:—

About seven acres in the park at Rothamsted have been set apart for the experiments, and divided into plots. Two of these have been left without manure from the commencement; two have received ordinary farm-yard manure, whilst the remainder have each received a different description of artifical or chemical manure, the same being, except in special cases, applied year after year on the same plot.

Referring first to the *Agricultural Results*, it may be premised that, *without manure*, the produce of hay has varied from year to year, according to season, from about 8 cwt. to nearly 39 cwt. per acre, and the average yield has been about 23 cwt. per acre per annum. On the other hand, the plot the most heavily artificially manured, and yielding the highest amount of produce, has given an average of about 64 cwts. of hay per acre per annum, with a variation from year to year from under 40 cwts. to nearly 80 cwts. Intermediate between the extremes here quoted, very great variation in the amount of produce has been exhibited on the other differently manured plots.

With these great differences in the amounts of produce the botanical character of the herbage has varied most strikingly. Thus, starting with perhaps fifty species on the unmanured land, any kind of manure, that is, anything that increases the growth of any species, induces a struggle, greater or less in degree, causing a greater or less diminution, or a disappearance, of some other species; until, on some plots, and in some seasons, less than 20 species have been observable, and on some, after a number of years, no more than this are ever traceable.

Even in the first years of the experiments it was noticed that those manures which are the most effective with wheat, barley, or oats, grown on arable land, that is, with gramineous species grown separately, were also the most effective in bringing forward the grasses proper, in the mixed herbage; and, again, those manures which were the most beneficial to beans or clover, the most developed the leguminous species of the mixed herbage, and *vice versâ*. It was further observed that there was great variation in the predominance of individual species among the grasses, and also among the representatives of other orders. And even in the second year the differences in the flora, so to speak, were so marked, that a first attempt at a botanical analysis of carefully taken samples of the produce of some of the plots was then made; in the third year more detailed separations were made; and, taking advantage of the experience thus gained, pretty complete botanical analyses have since been conducted four times, at intervals of five years, during the course of the now twenty-four years of the experiments; and on several other occasions partial separations have been undertaken. The character and tendency of the results so obtained may be very briefly indicated as follows:—

In the produce grown continuously without manure the average number of species found has been 49. Of these, 17 are grasses, 4 belong to the order Leguminosæ, and 28 to other orders. The percentage by weight of the grasses has averaged about 68, that of the Leguminosæ about 9, and that of species of other orders 23.

In the produce of the plot already referred to as the most heavily manured, and yielding the heaviest crops, the average number of species found has been only 19, of which 12—13 are grasses, one only (or none) leguminous, and 5—6 only represent other orders; whilst the average proportions by weight have been—of grasses about 95 per cent., of Leguminosæ less than 0·01 per cent., and of species representing other orders less than 5 per cent.

On the other hand, a plot receiving annually manures such as are of little avail for gramineous crops grown separately in rotation, but which favour beans or clover so grown, has given on the average 43 species. Of these 17 in number are grasses, 4 Leguminosæ, and 22 belong to other orders; but by weight the percentage of grasses has

averaged only 65—70, that of the Leguminosæ nearly 20, and that of species belonging to other orders less than 15.

With such very great variations not only in the amount, but in the botanical character of the produce of a crop under any circumstances so complex, it might be anticipated that there would be very great differences in its chemical composition, partly due directly to the supply of constituents by manure, partly to variation in the description of plants encouraged, and partly to the character and degree of development and ripeness of the varying components of the mixed herbage, according to the season and to the manure employed.

With a view to the elucidation of this part of the subject, the dry matter and the ash have been determined in the produce of every plot in every season, the nitrogen in that of all the plots for many of the seasons, and in some cases the amount of it existing as albuminoids has been determined. In selected cases also comparative determinations of "crude woody-fibre" and of crude fatty matter have been made. About 120 complete ash analyses have been executed. And, lastly, samples of the soil of every plot, in some cases at different periods, and in most cases representing the first, second, third, fourth, fifth, and sixth depths of 9 inches, or in all to a total depth of 54 inches, have been collected, and these have been chemically examined in various ways.

It is found that there is a considerable difference in the percentage of dry substance in the produce, and very considerable difference in the percentage of mineral matter (or ash) in that dry substance. There is still greater difference in the percentage of nitrogen in the dry matter, and again, a greater difference still in the percentage of individual constituents of the ash. When indeed it is remembered that a plot may have from 20 to 50 different species growing upon it, each with its own peculiar habit of growth, and consequent varying range and power of food-collection, it will not appear surprising that different species are developed according to the manure employed; and, this being so, that the character and amount of the constituents taken up from the soil by such a *mixed herbage* should be found much more directly dependent on the supplies of them by manure than is the case with a crop of a single species growing separately.

In further illustration it may be mentioned that, not only does the percentage of nitrogen in the produce of the different plots vary considerably, but the average annual amount of it assimilated over a given area is more than three times as much in some cases as in others. Again, the percentage of potass in the dry substance is more than double as much in some cases as in others; whilst the difference in the average annual amount of it taken up over a given area is more than five times as much on some plots as on others, dependent on the supplies of it by manure, and the consequent description of plants, and amount

and character of growth induced. The percentage and acreage amounts of phosphoric acid also vary very strikingly; and so again it is with other mineral constituents, but in a less marked degree.

It will be seen that in the history of so many of what may be called natural rotations, we can hardy fail to learn much that is of interest, not only in reference to the growth of the mixed herbage of permanent grass land, but also something of the relative positions of the different plants that are grown separately, in alternation with one another, in the artificial rotations adopted on arable land.

The botanical results are, moreover, of much independent interest, both by the facts which they already contribute, and by the incentive and direction they may give to future research.

Lastly, the chemistry of the subject will be found to offer many points of interest, in regard to the variation in the percentage composition of the produce, according to the manure applied; to the description of plants developed, and to the character of their development; to the availableness of the constituents artificially supplied; and to the amount and limit of the natural resources of the soil, both actually and compared with the results obtained when individual species are grown in arable culture.

It will be readily understood that the record, and the discussion, of the agricultural, the botanical, and the chemical history of about 20 plots, in 20 different seasons, must involve much detail; and although it is obvious that facts special to any one of the three main divisions of the subject may require for their elucidation reference to those of one or both of the others, it is still believed that it will conduce to clearness, and reduce unavoidable repetition, to maintain the divisions proposed as far as possible.

On the present occasion, Part I only, entitled—"The Agricultural Results," is presented. In Part II, "The Botanical Results," which will next follow, Dr. M. T. Masters, F.R.S., is joint author.

XXIV. "On the Comparative Structure of the Cortex Cerebri." By Bevan Lewis, L.R.C.P. Lond., Assistant Medical Officer, West Riding Asylum, Wakefield. Communicated by Dr. Ferrier, F.R.S., Professor of Forensic Medicine, King's College, London. Received June 18, 1879.

(Abstract.)

The object held in view throughout this investigation has been that of instituting an inquiry into the minute structure of the cortex of the brain in a prominent member of the Artiodactyla, and to critically compare the results with those obtained from an examination

of corresponding structures in certain members of the Ruminantia, Carnivora, Quadrumana, and also in the human brain. For this purpose the cerebrum was examined in the pig, sheep, cat, ocelot, and Barbary ape. In all cases the hemispheres were sliced from end to end in the fresh state upon the freezing microtone, and the sections examined *seriatim*, both fresh and after preparation by a process already described.* The regions to which special observation has been directed, are those over which a five- and a six-laminated type of cortex prevails, and which, in the members of the Ungulata examined, include the upper arc of the great limbic lobe (*gyrus fornicatus*), the frontal lobe, and the great mass of convolutions entering into the formation of the parietal lobe.

In this brief abstract I propose merely to enumerate the general conclusions arrived at, and which may be stated as follows :—

1st. In the Ungulata, as in higher Mammals, the cortex of the brain exhibits an extensive six-laminated region and a less extensive area constituted of five layers of nerve cells. These layers in the six-laminated cortex consist, as in higher animals, of a peripheral cortical layer, a second layer of small pyramidal cells, a third layer of large pyramidal cells, a fourth of small pyramidal and angular elements, succeeded by the large cells of the ganglionic series, and lastly the layer of spindle cells. The divergencies observed in various groups of animals are not dependent upon the general conformation and constitution of these layers, so much as upon differences observed in the essential characters of their elements and their mode of distribution.

2nd. In the five-laminated type, the second band of small angular and pyramidal cells is absent and the ganglionic layer assumes characteristic features.

3rd. Transition realms occur betwixt both laminated types in which the special features characteristic of the ganglionic series are in direct proportion to the thinning out and disappearance of the second band of angular elements.

4th. The ganglionic series in these members of the Ungulata differs from that of higher Mammals in the conformation of its individual elements and their distribution.

As to Conformation. The cells are more elongated, pyramidal in contour, and never assume the same average dimensions as those of the cat, ocelot, and ape. The swollen plump cell of these animals is never seen in the pig and sheep.

As to Distribution. They are scattered over a comparatively far wider area of the cortex.

5th. The pyramidal form assumed by these cells is, from reasons stated in the original paper, significant of far less complex relation-

* "Brain," a Journal of Neurology. Part iii. 1878.

ships through their branches, the latter being far more numerous in the swollen irregular cells of higher Mammals.

6th. The pyramidal cells of this layer closely approach in their dimensions and contour the larger pyramidal cells of the third layer in man and the ape, and may be assumed to work upon the same physiological level.

7th. The ganglionic series in these members of the Ungulata, assumes the clustered arrangement which is seen most highly developed in the motor area of the brain of the higher Mammalia. These clusters, or nests of Betz, are in the pig and sheep more densely packed with cells than in higher animals, cover oblong or oval areas of some depth, and in the most highly developed region of this lamination are so closely approximated that they tend to become confluent and assume most distinctive features.

8th. The regions over which the five-laminated cortex, with its nested cells, spreads, are for the Ungulata the anterior half of the great limbic lobe (superior arc), the whole of the frontal lobe, and the lower parietal gyri.

9th. The chief point of interest as regards the five-laminated cortex, is the fact that the whole ganglionic series throughout this extensive area maintains a remarkable uniformity of the magnitude of its elements, and in the complexity of their relationships.

10th. This fact is highly significant, since, upon localising the corresponding stratum in the cat and the ocelot, we find its grand development limited to the immediate neighbourhood of the crucial sulcus. On the other hand, in the ape and in man we find over a wide area of the cortex the same type of formation, but, far from there being general uniformity in the size and complexity of its cells, there is exhibited here an exceedingly rich development of this layer in the paracentral lobule of Betz, and the centre for the movements of the hand and arm, whilst other portions of the motor area are less conspicuously developed the more distant they are from these areas.

11th. These anatomical facts enable us to conclude:—

(*a.*) That in the Carnivora the motor centres are concentrated, so to speak, upon the limited area occupied by the limbic and parietal boundaries of the crucial sulcus.

(*b.*) That in the Quadrumana and Man they are more widely scattered and betray great divergence in developmental complexity at different sites.

(*c.*) That in the Ungulata they are still more widely scattered, but exhibit at all points a most noteworthy uniformity in their structural details and groupings, assuming, at the same time, a great resemblance to the elements of the third layer of the cortex in the higher orders of Mammalia.

12th. It is, therefore, inferred, that dissociation of centres plays an

important part in the explication of fundamental divergencies in the *rôle* of cerebral activity—thus, close concentrated association of centres is maintained in those animals where the associated movements of vast musculatures are required to act in concert for sudden and supreme efforts in the struggle for existence—hence the peculiar structure of the motor area in the cat and ocelot.

The dissociation observed in man and the ape appears to point to the comparative independence of special musculatures, to the possibility of their higher education, and (in the great disproportionate complexity of certain areas) to the pre-eminently high and complex relationships of these special centres and the musculatures which they represent.

The widespread dissociation of centres observed in the species of Ungulata examined, points, on the other hand, to a comparative simplicity in their resultant activities—there being in these animals no special demand for the collective and complex associations in action of widely separated musculatures.

13th. The results of the investigation lend still stronger confirmation to the belief that these anatomical units of the ganglionic layer are especially concerned in the production of those complex movements, which Ferrier has shown to be the special functional endowment of the motor area; and it is highly probable that a study of the dimensions, distribution, and relationships of these cells in the lower animals will serve to elucidate, in some degree, the diversified phenomena resulting from the excitation of this important area of the cerebral cortex.

Note.—In a recent contribution ("Psychiatrisches Centralblatt," Nr. 6), Meynert, referring to the discovery by Betz of the pyramidal cells in the motor area, attributes to it no importance. He states that the size of these cells depends simply upon the distance pursued by their processes towards the outer zone of the cortex. I need scarcely point out the self-evident fallacies involved in this line of argument. Suffice it to say, that such a rule cannot be one of universal applicability, since, although the size of these cells as a general fact increases with their depth, yet, from the second down to the fourth layer, we find numerous pyramidal cells no larger than those of the second layer. Again, over many regions of the cortex, the cells of the third layer are often larger than those of the fourth. In these cases, therefore, Meynert's assumption is untenable. Further, these cells are often larger at the sides of a gyrus and the bottom of a sulcus than beneath the summit of the convolution, *i.e.*, at a shallower portion of the cortex than at a deeper site. As this theory of Meynert does not explain the very specialized form, relationships, or arrangement of these elements, nor provide for the constant and extensive exceptions to be taken to his rule, the above considerations, together with others which must occur to a candid and careful inquirer, will prove wholly subversive of such a doctrine.

XXV. "An Account of Experiments on the Influence of Colloids upon Crystalline Form, and on Movements observed in Mixtures of Colloids with Crystalloids." By WILLIAM M. ORD, M.D. Lond., F.L.S. Communicated by J. SIMON, C.B., D.C.L., F.R.S. Received June 4, 1879.

[PLATE 1.]

In the "Proceedings of the Royal Society" for 1866, Dr. Montgomery has recorded some very remarkable observations made by him on the behaviour of "myelin" when brought into contact with water. "Myelin" being a term of various applications, it is necessary to state that the substance indicated in Dr. Montgomery's statement is an alcoholic extract of yolk of egg. A small quantity being placed on a microscope-slide, and covered with thin glass, is brought under observation by a tolerably high object-glass. The mass is shapeless, and indistinguishable from crushed grease till the addition of water, when a multitude of minute rounded processes begin to sprout from all parts of the surface exposed to the action of the water. The processes are, when fairly formed, tube-like, with bulbous or simply rounded ends, have a thick, refractile contour, and apparently fluid contents. They remind one of nerve-tubes, or, still more closely, of the tubular and rounded projections which are seen, under the microscope, to issue from the cut ends of nerves in contact with water. Dr. Montgomery shows in the same paper that, if albuminous fluid be used instead of water, spheroidal protrusions are formed, and float free.

Recently, in the examination of a biliary calculus containing much cholesterin, my attention has been drawn to some very remarkable changes of form undergone by that substance under certain conditions—changes so important in their resemblance to phenomena taking place in minute living organisms, that I am induced to give an account of them. It will be seen that they belong partly to the class of changes described by Dr. Montgomery, as above quoted, and partly to those described by Mr. Rainey in his well-known work on "Molecular Coalescence."

Pure cholesterin crystallises, as everybody knows, in the form of colourless rhombic plates. It is insoluble in water and most saline solutions, but is supposed to be held in solution in the bile by taurocholic acid or the soda salt of that acid. In biliary calculi it is found for the most part crystalline; but at the surface of calculi and in biliary gravel it often occurs in lustrous spherules.

Experiment 1.—An ethereal solution of cholesterin is added to fresh egg-albumen in the proportion of one part to two. After good shaking in a test-tube, the mixture becomes gelatinous, and is left for twenty-four hours in the test-tube, or for any period exceeding half an hour, in an evaporating dish to allow of free escape of the ether. As the

ether passes off, the cholesterin is deposited in the substance and on the surface of the albumen. The superficial deposit is partly crystalline, but the deposit in the albumen is entirely spherical or acicular. The crystals on the surface are rarely perfect, and generally have rounded angles (Plate 1, figs. 1, 2). The spheres are of all sizes up to $\frac{1}{500}$ inch diameter, and are at first homogeneous and singularly lustrous (fig. 17). But they speedily begin to undergo internal disintegration. Becoming cloudy, they are soon made out to be composed of fine short needles, packed together like very closely cropped hair, brushed in a direction running from one pole to the opposite (figs. 3—11). Subsequently they break up into tufts of hair-like needles, radiating in a brush from the pole of divergence, and these tufts expand and grow till each of them becomes a long curl or ringlet of wavy hair (figs. 10, 11). By the end of a week nearly all the cholesterin has come to this state of long curling fibrils, and the whole process may remind the observer of the growth of minute confervoid filaments from spores.

A new series of phenomena is observed when the albumen, having the cholesterin immersed in it, is treated with glacial acetic acid. But before describing this change, the behaviour of cholesterin in presence of acids may be described.

Experiment 2.—Treated with nitric acid cholesterin loses its crystalline form with some effervescence, and swells out irregularly at its surface, throwing off oily-looking spherules. Finally it is entirely resolved into such spherules.

Experiment 3.—Treated with hydrochloric acid it undergoes little or no change.

Experiment 4.—On the addition of glacial acetic acid to pure cholesterin, the rhombs are seen under the microscope to undergo an instant change of structure. They break up internally into crystalline fibres, usually springing from one of the angles; and they emit radiating tufts of sharp-pointed needles which seem to grow at the expense of the substance of crystals. If, now, the slide be heated till the acid begins to boil, the cholesterin is wholly or in part dissolved; but, as the slide cools, acicular crystals shoot out from any portion of cholesterin that is left undissolved, or from new centres, and rapidly cover the field. These crystals during their growth give the impression of their being protruded like spears. They are often as thick as the spicules of fresh water sponge, and as refractile. They are solid. The cholesterin appears in this case to be chemically altered by the acetic acid, combining with it to form a sort of ether.

Experiment 5.—Some pure glycerin being added to such a preparation as is produced in the preceding experiment, during cooling, at any point where the glycerin meets the growing spears it breaks them up wholly or partially into globules, making them look as if they had been put into the fire and fused.

Experiment 6.—If glycerin be mixed in about equal quantities with the acetic acid and cholesterin while still on the slide, and if heat be applied, after the covering glass is in place, till bubbles form rapidly, the slide instantly examined is found covered with spheres, some as large as fat cells and quite regular, others egg-shaped, and others aggregated into masses (fig. 12). The masses are dense and very brilliant. As cooling proceeds all the spheres break up. The large ones contract at their surface, and seem thereby to squeeze out fluid from the interior, which takes at first the form of tubes, afterwards of spheres. An active movement of this kind often results in the formation of a group of small spherules in the place of the large one, looking like a glomerulus of spores (figs. 13, 14). Presently these in turn change each instantaneously into needles, the change proceeding as follows:—a single spherule darts away from the mass; as it goes it leaves a stiff raphis in its course, and it is itself proportionately diminished in size, till its last fragment forms the point of the raphis (figs. 15, 16). The ball is, so to speak, unwound into a stiff thread of glassy substance. Here and there spheres break up, without change of place, into variously shaped tufts of needles.

Experiment 7.—Acetic acid is added to cholesterin and albumen, and the mixture is heated to boiling. Examined now as quickly as possible the mixture presents spheres and tufts of needles rapidly growing in the fluid part of the field; spheres and spheroids and combinations of most fantastic shapes in the transparent albumen. The spheres are at first perfectly homogeneous and circular in outline (figs. 17—26). After a time they become oval and gradually a little pointed at the ends. Then usually a little globular protrusion forms at one end or both; then a zone of indentation is drawn round the oval, usually at about one-third of the length of the surface from the protrusion; the interior next gives indication of a subdivision of the substance of the ovoid into two, three, or four mutually compressed spheroids; and at last the whole changes, with magical suddenness, into a crystalline mass of obscurely rhombohedral shape. In process of time this mass breaks up yet further into raphides. The great transformation is attended by upheavals and depressions of the surface, causing a general appearance of singularly life-like convulsions and contortions of the mass. All the change is most rapid during cooling, but it continues slowly for hours and days subsequently till every particle of cholesterin has come to the needle form. If solutions of gelatin be used instead of albumen, similar results are observed. In the gelatin spheres are readily formed, and slowly turn into needles. If acetic acid be added and heat applied the rapid conversion of spheres into needles, attended by active convulsive movements, can be conveniently watched in the cooling mixture; and if glycerin be used as well a great variety of lively transformations may be witnessed.

This series of observations has, in the first place, some importance as involving a rapid mode of exhibiting the influence of colloids upon crystalline form. In Mr. Rainey's original processes, and in the later experiments made by Harting, of Utrecht, and by myself, the formation of spherules of crystalline matter within colloids has been effected only on a microscopic scale, and in the most gradual way. And although Professor Guthrie has recently, in independent and original experiments, obtained large spherical masses of sulphate of copper by the slow evaporation of solutions of that substance mixed with gelatin, it has not been possible to watch so continuously the steps of the process. But here, within a short compass, we are able to see the crystalloid cholesterin assume, when first deposited within the substance of the colloid, perfectly spherical form; we can follow the gradual emancipation of the crystalloid from the control of the colloid and its return to a quasi-crystalline form within the albumen, and to definite crystals within the gelatin or glycerin with acetic acid. We may infer that the crystalloid is not so intensely crystalloid in its tendencies when first deposited as afterwards. We may infer also that the power of the colloid is, under certain circumstances, when coagulation is not produced, intensified by heat; and, in the loss of controlling power experienced by the cooling colloid, we may see a parallel to the loss of power experienced by a degenerating colloid, for instance in the transition from mucous to horny matter. Where the changes are most rapid, as in the heated mixture of cholesterin, acetic acid and glycerin, the transformation is seen to be accompanied by vigorous intestine movements. To review this observation, we have before us in such a mixture, when cooling has only just commenced, perfect lustrous spheres looking like drops of oil. Of a sudden these are affected by peristaltic movements, and, as the surface contracts, the mass is resolved into minute spheres. The heat and the crystallinity have balanced each other for a time, but when the balance has been lost and the process of crystallisation has commenced, the first step has been not the formation of crystals, but of smaller spheres. So long as all was quiet, the large sphere was possible; but when molecular vibrations were set up as the attendants of crystallising separation, the molecules at opposite ends of poles were withdrawn from their power of holding one another in position at such a distance, and the general shaking allowed only of the existence of small spheres, the constituent particles of which were all near enough to each other to allow of each exercising strong enough attractive influence on its fellow to resist the distracting influence of the vibrations set going by the crystallising force. Instead, therefore, of one centre around which, or through which, all attractions were balanced, we have a number of centres with spheres drawn round them proportionate in minuteness to the activity of the perturbing force. The process, in one aspect,

may be compared to the whipping of an egg. In another, it may be compared with the cleavage of the ovum after impregnation. In the ovum up to the time of impregnation the contents of the zona pellucida form one sphere. After impregnation, after the introduction of a material having the power of inducing new activities, shrinkage is followed by movements having the character of a gradual peristalsis, and subsequent fissions reduce the sphere to the mulberry mass of small equal spherules.

Regarded from another point of view, we see a series of movements resulting from the tendency of matter to come to rest. We see stages which may be compared to the spring of the bow when the drawn string is let go; and, again, to the straightening of the unstrung bow. Or we may see in the chain of movements, beginning with the formation of the sphere and ending in the crystal at rest, a parallel to the chain of force formed when water is raised by the sun to the clouds and hill-tops to descend in rain or rivers to the sea—to rest; or to the chemical chain in which oxygen and carbon, torn asunder by light within vegetables, slowly recompose carbonic acid within animals, and in turn come to rest.

Here, again, the question is suggested to me, how far may the simpler movements of the constituents of animal and vegetable organisms be due (among other things) to the struggle of one group of substances against the grasp of another, to the tendency of each substance to ultimate complete separation from the substances with which it is intermixed, and to the tendency of each separated substance to assume the nearest possible approach to a static condition?

The observations have, however, an immediate practical relation to the structure of such biliary calculi as are composed chiefly of cholesterin. Such calculi are seen in sections to be composed of several distinct stratifications. Most commonly they have (1) a thin rugged outer layer, deeply tinged with bile colour; within this (2) a brown portion marked with concentric lines corresponding to laminæ; within this (3) a lighter-coloured layer, presenting radiating markings or distinct radiating fissures between crystalline columns; and, lastly (4), at the centre a mass of deep colour and friable consistence (fig. 37). The strata are not sharply defined from each other, but are joined by transitional layers. They differ severally in minute structure. In the outer crust (1) distinct independent spherules and spheroidal masses composed of cholesterin, mixed with mucus and bile-pigment, can be seen (figs. 27—31). In the concentric layers (2) the microscope shows a substance pretty uniformly inter-penetrated by, and partially coloured with, pigment; it is marked with radiating crystalline fibrations of varying degrees of fineness running sinuously at right angles to the concentric markings; in fact, it shows a combination of concentric and radiating arrange-

ment. The light radiating layer (3) is composed of nearly colourless crystalline columns of cholesterin radiating as from the centre of the calculus, with accumulations of pigment in the spaces between them; while the centre (4) is nearly all deep brown pigment, actually black in some cases, mixed here and there with perfectly formed, thick, colourless, rhombohedral slabs of cholesterin. The explanation of this composite structure appears tolerably easy by the light of the experiments recorded. The cholesterin is first deposited in the pigment with or without some mucus. These, the colouring matter in particular, are substances of high molecular composition, and when first secreted in the bile are most typical colloids. But when kept outside the body, or retained for any length of time within it, the bile-pigments slowly exchange the colloid for the crystalline state, and may often be seen in rhombohedral forms of great beauty in hydatid cysts in the liver. In proportion as the pigment is relatively abundant at the time of the precipitation of the cholesterin the latter is more completely fused and intermixed with it. The superficial part of the calculi, of course the latest deposit, shows this intermixture. The concentric layers show a commencement of rearrangement of particles subsequent to deposit. Here the cholesterin and pigment are beginning to separate from each other, and the lines of crystallisation are already related radially to the centre of the calculus. The calculus exhibits on a large scale the mingling concentric and radiant arrangement of molecules seen in the small spheres of carbonate of lime and other salts formed by Mr. Rainey, and in the small spheres found on the surface of the calculi under investigation (see fig. 30). In the light-coloured radiating layer (3) separation has gone further; crystalline fibration has been exchanged for strongly-marked angular and columnar form, and the colour is nearly all interstitial to the crystals, while the whole arrangement is again clearly related to the centre of the whole sphere. Lastly, in the very centre (4), where all lines of attraction are balanced, the pigment accumulates in quantity, and the few crystals left are independen and typically perfect in form. The result of the investigation is to the remarkable effect that a metamorphic change is in process within the calculus from the time of its first formation up to the time at which it is submitted to examination, involving a constant rearrangement of particles in relation to the centre of the whole mass, and dependent upon the double tendency seen in cholesterin under experiment: first, the tendency to be deposited in the spherical form when precipitated in a colloid matrix; second, the tendency to revert, in process of time, to a crystalline form: the molecules in their passage from the one state to the other being caught, as it were, by the common attraction of the great sphere, and brought to a certain extent under their dominion. The fact that the colloid forming the matrix is one which tends ultimately to become

crystalloid is probably one reason why the metamorphic changes are more rapid and are pushed further than in urinary calculi.

There is, of course, a considerable variety in the proportions of the various structures in these biliary calculi. Sometimes a gallstone shows entirely a radiating structure. It is then usually but little stained with pigment. Sometimes it is nearly all concentrically marked; the pigment is then abundant. I have throughout spoken more of the pigment than of the mucus as the modifiying colloid, since the examination certainly brings it into greater prominence; just as I have elsewhere shown that the chemically allied pigment of urine exercises a parallel effect upon the form of uric acid, by preventing that substance from assuming the rectangular form proper to it in its pure state. The calculus is to all intents and purposes a dead thing within the living body, yet it is of great interest to consider that it is the seat of slow metamorphic changes, and that these changes are effected in and by substances characteristically belonging to animal life, and that they simulate processes seen in living structures, processes that have been regarded as purely vital and dependent upon the influence of cells or nuclei.

I have next to speak of some allied phenomena exhibited by uric acid. That substance, under certain modes of treatment, manifests a succession of colloid and crystalloid states and attributes parallel on the one side with those of the colloid pigment on the other with those of the crystalloid cholesterin.

Experiment 8.—If some uric acid be boiled for some time with strong solutions of chloride of sodium or of phosphate of soda, and the solutions be left after filtration to crystallise, urate of soda is deposited. But not in crystals. The fluid, after being thoroughly cooled, is seen to be nearly filled with a semi-transparent gelatinous-looking deposit, reminding one of the mucus separated from the urine of early vesical catarrh. This deposit consists, as seen under the microscope, of a gelatinous-looking matrix through which a number of bright but as yet formless points are scattered. After a few hours a slight diminution of bulk is apparent, and the microscope then shows spherical arrangements of bright granules, and bright spherical tracts without structure suspended within the jelly, the bright granules being still present. Slowly, from day to day, the bulk of the precipitate grows less, and it gradually assumes a whitish instead of a transparent aspect. The spherical bodies are gradually resolved into spherical tufts of needles radiating from a common centre; the matrix becomes more and more granular. At last only a little white powder is left lying at the bottom of the liquid; it contains no trace of matrix, and consists entirely of needles, some in tufts, some loose This condition is not attained in temperate weather before the end of a fortnight or three weeks.

The whole phenomenon suggests at first sight that a strong crystalline solution can act on another crystalloid deposited within it, just as a colloid would act. But the microscopic examination demonstrates that the urate is deposited at first in the form of a colloid, and in its subsequent metamorphoses, which are gradual and distributed, acts in one part as a colloid, in another as a crystalloid. Uric acid is known to be thrown down by acids in what has been called a hydrated form, which is clearly also a colloidal form, and can be seen to be at first structureless, though resolved speedily into crystals. Urate of soda appears also to have its colloidal stage, in ordinary conditions of short duration, but prolonged by the action of the strong saline solution. The explanation then is, that the saline solution retarding metamorphosis allows of a long colloid stage, during which the slowly-formed crystals are modified by that part of the substance which remains still colloidal.

It appears to me that by means of this sort of process the existence of a colloid (nascent) stage in the natural history of many crystalloid substances of high molecule may be sought for and studied. In this way it can be shown that uric acid, when deposited from solutions of urea of certain strength, passes through colloid and spherical stages. With solutions of less than 4 per cent. strength of urea, uric acid, if deposited from the boiling solution, forms fine rectangular crystals. But if solutions of 5 per cent. strength and upward be used, the gelatinous deposit and the spheres are abundantly produced. When I noticed this for the first time, I thought it might be urate of ammonia, resulting from the decomposition of some of the urea. But as it was deposited from acid solutions this was not probable. I then collected a considerable quantity of the deposit, and submitted it to organic analysis. My friend, Mr. John Moss, F.C.S., made several determinations for me; but they did not yield any definite results. And as the deposit occurs with solutions of very decided acidity, I have been inclined to give up a second hypothesis which I had formed —that the spheres were urate of urea. If they be uric acid, it is noteworthy that the colloid stage is shorter than that of urate of soda in the saline solutions.

I have already alluded to Dr. Guthrie's masses of sulphate of copper as illustrating on a large scale Mr. Rainey's original observations. They are, however, formed but slowly. I am now able to bring before the Royal Society an experiment, in which spheres of crystalloid matter can be rapidly formed, and of such a size as to be readily visible to the naked eye.

Experiment 9.—If to a solution of urea of from 1020 to 1025 specific gravity strong nitric acid be added in equal bulk, the mixture, at first hot, deposits on cooling to 50° F. crystals of nitrate of urea in the form of separate and distinct brilliant flakes, which subside at once.

Experiment 10.—If a little albumin be added to the solution of urea and if then to the filtered liquid the equal bulk of nitric acid be added as rapidly as possible, the albumin is only partially precipitated, and, after a little warming, is all re-dissolved. In the resulting clear straw-coloured liquid there appear during cooling spherical bodies, suddenly suspended at various depths. They look very much like little globules of solidified fat, and grow rapidly from the size of a mustard-seed to the bulk of a pea. Each is composed of crystalline fibres radiating from the centre, and whereas the deposit in the previous experiment does not occupy more than one-third of the fluid, these fill the whole bulk of the fluid and, mutually compressing each other, give the contents of the tube in which the experiment is made the appearance of a bar of beautiful fibrous spar. The experiment occurred to me first in the examination of concentrated albuminous urine, but it succeeds equally well with pure solutions, and forms, I submit, the most brilliant illustration of Mr. Rainey's valuable work hitherto made known.

DESCRIPTION OF PLATE 1.

Figures 1 to 11. Cholesterin in albumin: 1, 2, on surface; 3–11, successive stages of forms observed in the cholesterin deposited within the substance of the albumin.

Figures 12 to 16. Cholesterin after being heated with acetic acid and glycerin: 12, sphere first formed; 13, sphere in act of contracting and exuding smaller spheres and tubular processes; 14, mass of globules like mulberry resulting from subdivision of sphere; 15, 1, 2, 3, 4, diagrammatic representation of transformation of globule into raphis; 16, a mulberry mass in act of transformation into a fan of raphides.

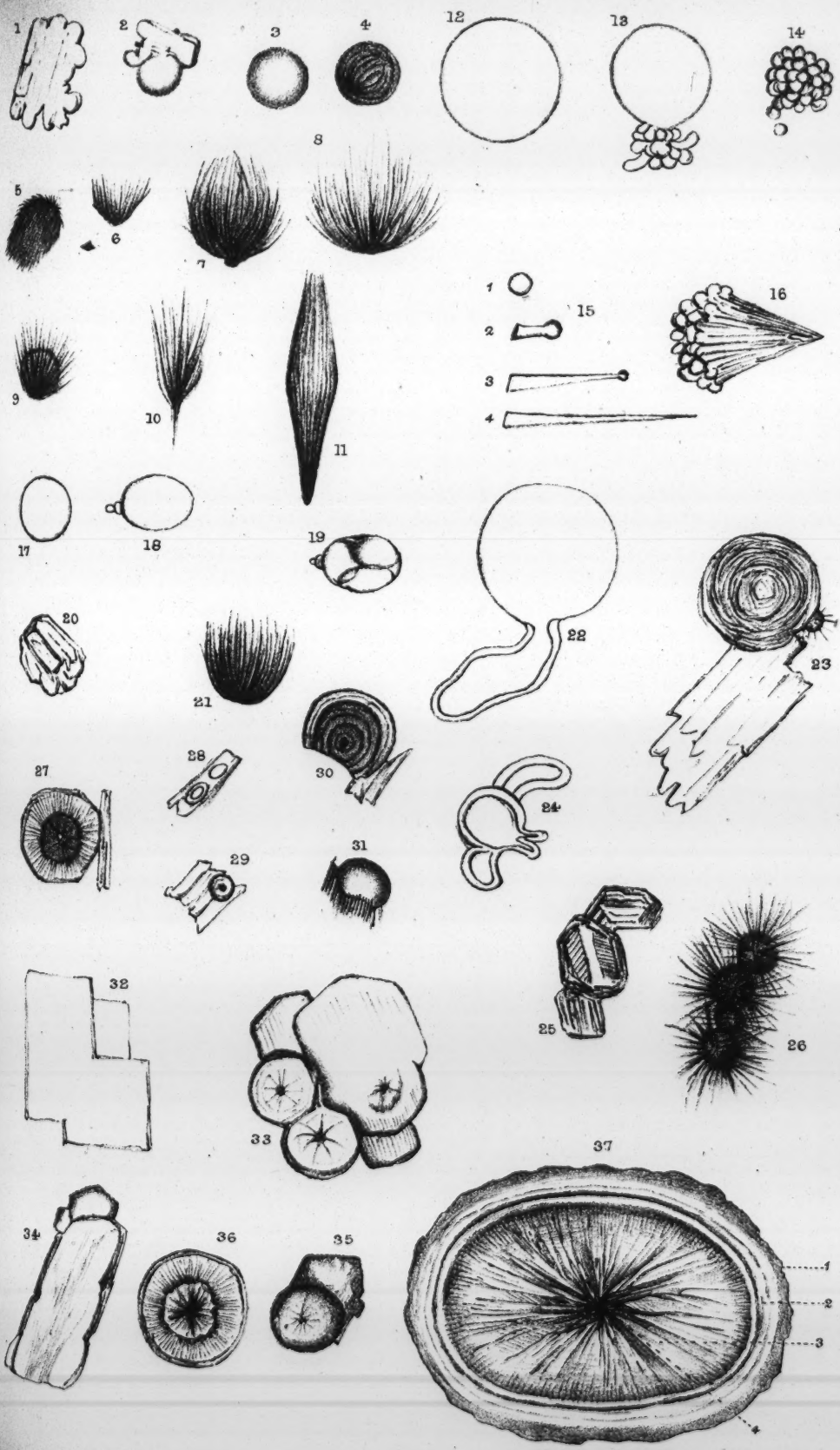
Figures 17 to 26. Shows three kinds, 17–21, 22 and 23, 24–26, of metamorphic change accompanied by movements undergone by cholesterin after heating with acetic acid and albumin.

Figures 27 to 31. Spheres of cholesterin mixed with crystalline matter, from the surface of a biliary calculus.

Figures 32 to 36. Forms seen after solution of a calculus in ether and slow evaporation of the liquid in contact with the pigment. They are transitional from crystal to sphere.

Figure 37. Diagrammatic section of Biliary Calculus. 1, outer crust dark-coloured without either concentric or radiating structure; 2, second concentric layer; 3, radiating crystalline layer; 4, centre, pigmentary, structureless.

Proc. Roy. Soc. Vol. 29. Pl. 1.



West, Newman & Co. lith.

XXVI. "Report to the Committee on Solar Physics on the Basic Lines common to Spots and Prominences." By J. Norman Lockyer, F.R.S. Communicated at the request of the Committee. Received June 19, 1879.

I have now for some time past been making efforts to bring together the various observations which have been recorded of the lines visible in solar disturbances at the sun's limb, and those observed to be widened, brightened or otherwise modified in the spectra of solar spots.

The result which stares us in the face when we examine the lists of the lines given by Young as representing his Sherman work is so striking, both with regard to the conclusions which we are justified in drawing from the old work, and the suggestions for future operations, that I have thought it desirable to call attention to it without waiting until the Italian observations have been discussed.

Although Young's observations of the chromospheric lines extend over the whole visible spectrum, the list of lines in the solar spots is limited to the region between B and *b*, I have therefore limited the discussion to this region.

As a basis for the discussion, I have used the lines given in Thalén's admirable tables, comparing these lists with the lines shown in Ångström's and Thalén's map, and indicating the intensities of the lines which are given in the tables, and which particular line occurs in the map only.

Further, as a discussion of the coincident lines shown in some sheets of Ångström's map, which I communicated to the Royal Society last March, had convinced me that the lines shown to be common to two or more substances by Thalén were not due to impurities ("Proceedings of the Royal Society," No. 194, 1879), I have shown these coincidences, where they occur, in a special column.

Having then Thalén's list for a basis, other columns showing us whether each line is seen in spots and prominences, and how it is affected, give us in one view, for each metallic substance, exactly what happens to the lines of that substance. We see at once, for instance, which lines are not touched, those again which are visibly affected both in spots and storms, or those recorded in one table and not in the other.

Taking all the lines included in the discussion, the following statistics will show how they are distributed:—

Total number of lines in Thalén's list and map included in the discussion..... 345

Number of lines affected in spots..... 108
Number of lines bright in storms 122
Number of lines common to spots and storms 68
Number of lines seen in neither spots nor storms ... 183

So much for the list of lines as a whole: the following list shows the number of lines assigned to each metal, the number of lines which occur in both spots and storms, or only in one or the other, and the number of lines unaffected.

Metal.	Number of lines.	Number of lines common to spots and storms.	Number of lines due to		Unaffected.
			Spots.	Storms.	
Sodium.....	8	4	6	6	0
Magnesium.....	4	3	4	3	0
Barium.....	23	1	3	7	14
Calcium.....	25	7	15	10	7
Strontium.....	18	0	0	0	18
Nickel.....	12	1	3	2	8
Cobalt.....	19	3	3	3	16
Manganese.....	16	2	3	6	9
Cadmium.....	15	0	0	0	15
Chromium.....	14	3	3	5	9
Titanium.....	87	11	18	18	62
Iron.....	104	33	50	62	25
	345	68	108	122	183

It will be seen that the ratio between the affected and unaffected lines is very variable.

What strikes one indeed in regard to all these substances is the wonderful irregularity in the behaviour of the various lines; there is no relation for instance between the widening of the lines in the spots and their appearances in the prominences. As I have before shown, there is no relationship between the intensities of the lines and their appearances in the spots and storms.

Amongst the substances shown in the table I have included strontium, in order that the difference in its behaviour in comparison with calcium and barium may be seen.

But all these, however, are matters for subsequent discussion; what does come out in the strongest way is the following very remarkable fact.

Among the 345 lines given by Thalén are 18 with identical readings in two spectra. I have before given reasons to show that these are not due to mutual impurities, they therefore are the exact equivalents of those lines which I have termed basic in my communication of last December to the Royal Society.

If they be basic, then we should expect a considerable development of these lines in the spectrum of the sun's reversing layer, which spots and storms enable us to study apart from the absorption going on at higher levels.

These tables enable us to settle this question, and they settle it in the most conclusive way. The only constant thing about the tables is, that these basic lines are always widened in the spots. However badly the brighter lines of a chemical substance may be represented amongst the spot lines, the basic line, which is often of the second or third order of intensity and sometimes even of the fourth, is never absent.

The same fact holds almost equally true with regard to the storms.

The following table gives a comparison of these coincident lines of Thalén's with Young's tables.

Comparison of Thalén's coincident lines with those seen by Young in solar spots and storms.

Thalén.			Young.		
			Spots.	Storms.	
Wave-length.	Common to	Intensity.	Widening.	Frequency.	Brightness.
5207·6	Fe Cr	3 1	4	10	6
5203·7	Fe Cr	3 1	4	10	6
5340·2	Fe Mn	2 3	2	1	2
6064·5	Fe Ti	2 2	3	5	2
5661·5	Fe Ti	3 1	4	15	2
5403·1	Fe Ti	2 3	4	5	3
5396·1	Fe Ti	2 2	7	4	2
5352·4	Fe Co	4 3	2	4	2
5265·8	Fe Co	2 3	2	10	4
5168·3	Fe Ni	3 5	4	40	30
5166·7	Fe Mg	2 1	2	30	20
5681·4	Fe Na	3 3	3	2	1
6121·2	Co Ca	1 3	4	5	3
5601·7	Ca Fe	4 1	2		
5597·2	Ca Fe	3 1	2		
5856·5	Ca Ni	3 4	2		
5425·0	Ba Ti	3 3	4		
6449·0	Ca Ba	2 3	2		

So far as my own knowledge of these matters goes, I can imagine no severer test to apply to the hypothesis which I have lately advanced, and to my mind the proof is conclusive that at the temperature of the sun's reversing layer, we have a mixed mass of vapours in which the base is more predominant than the chemical substances to which that base is common.

This result of the inquiry not only adds a new importance to the observations of spot and storm phenomena, but it suggests the precise work which should be done, both in the observatory and laboratory. Not only must special prominence be given to the brightening and thickening of these basic lines in spots and storms, but the existence or non-existence of them in other substances besides those indicated by Thalén, should be inquired into. This latter branch of work has indeed already been begun, since I have pointed out (in the "Proceedings," No. 194, 1879), in a table showing basic lines, that some lines in the blue and green seen many times by Young in storms, are common to many substances, the spectra of which were observed with a Rutherford's grating (second order, 17,000 lines).

In this work it will be well to include those lines which only differ by a tenth of Thalén's unit, although they have been rigorously rejected in the present discussion and tables. Young, indeed, included such lines, among his coincidences, the line at 5269·5 being a case in point which is widened in spots, and has been seen 50 times in storms.

Attention must also be directed to those lines which, like those shown in the following table, although seen both in spots and storms, have not yet been detected in the spectrum of any chemical substance.

List of lines seen more than ten times by Young in the storms, but which do not appear in Thalén's list or maps.

Wave-length.	Frequency.	Brightness.	Whether seen by Vogel in spots.	Whether seen in spots by Young.	Widening.
6515·5	15	4	Yes		
6453·8	10	6	Yes		
6429·9	20	4	Yes		
6346·1	10	4	Yes	Yes Ru. Ir.*	2
5990·0	10	4	Yes		
D_3 5874·9	100	90	Yes	Yes	2†
5275·0	30	15	..	Yes	2
5197·0	15	10			

* Ruthenium and iridium lines are not given in Thalén's list; the line in question is marked in the map without any remarks. In Ångström's map it is entered and marked as a line coincident with a line given by Ru, Ir.

† But sometimes reversed.

Iron.

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
6489·8	3	..	..	..	..	..	..	Seen by Vogel.
6407·0	Map	..	..	..	Yes	5	2	
6399·0	1	..	..	..	Yes	5	1	
6392·6	Map	..	..	..				
6357·7	Map	..	Yes	4				
6300·3	3							
6245·4	2	..	..	..	Yes	8	5	
6231·5	Map	..	..	..	Yes	5	1	
6229·7	2							
6199·6	Map	..	..	..	Yes	2	2	
6190·5	2	..	Yes	2	Yes	10	2	Vogel gives a line at 619·1.
6148·1	Map	..	..	..	Yes	3	2	
6135·6	2	..	Yes	3	Yes	2	1	
6064·5	2	Ti	Yes	3	Yes	5	2	Vogel gives a line marked iron at 606·4, much thickened.
6023·0	3							
6019·1	4							
6007·5	4	..	Yes	Double, nearly invisible in spot spectrum	..	..	..	Vogel gives a line marked iron at 600·7.
6002·1	4							
5986·2	4							

Iron—*continued.*

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
5984·2	4							
5982·8	4							
5976·1	4							
5974·6	4							
5913·2	Map	..	..	..	Yes	2	1	
5883·0	Map	..	..	..	Yes	2	1	
5761·9	3							
5708·3	3	..	..	..	Yes	1	1	Vogel gives a line marked iron at 570·8.
5705·1	Map	..	Yes	4	..	..	..	Vogel gives a line marked iron at 570·5.
5681·5	Map	..	..	..	Yes	2	1	Vogel gives a sodium line at 568·1.
5681·4	3	Na	..	..	..	..	..	
5661·5	3	Ti	Yes	4	Yes	15	2	Vogel gives an iron line at 566·1.
5657·6	1	..	..	..	..	..	..	Vogel gives an iron line at 565·8.
5654·4	3	..	..	..	Yes	2	1	
5623·2	3	..	Yes	2	Yes	2	1	
5614·5	1	..	Yes	3	Yes	2	1	
5601·7	1	Ca	Yes	2				
5597·2	1	Ca	Yes	2				
5591·2	2	..	Yes	2				
5585·6	1							
5585·5	Map	..	Yes	3	Yes	2	1	
5583·7	Map	..	Yes	4				

5574·9	2	..	Yes	2						
5571·7	1	..								
5568·5	2	..	Yes	2						
5531·6	Map	..	..	..						
5525·9	Map	..	..	..	Yes	40			5	
5505·9	3	..	..	..						
5500·5	3	..	Yes	2	Yes	2			1	
5496·6	3	..	Yes	2	Yes	2			1	
5486·8	4	..	Yes	3						
5462·3	Map	..	..	..	Yes	1			1	
5454·7	1	..	Yes	3	Yes	10			4	
5445·9	1	..	Yes	4	Yes	10			4	
5433·0	Map	..	Yes	4	Yes	2			2	
5428·8	1	..	Yes	3	Yes	8			3	
5423·6	Map	..	Yes	3						
5414·5	Map	..	Yes	3	Yes	2			2	
5410·0	Map	..	..	..	Yes	2			1	
5404·8	2	..	Yes	4	Yes	2			1	
5403·1	2	Ti	Yes	4	Yes	5			3	
5396·1	2	Ti	Yes	7	Yes	4			2	
5392·3	3									
5392·2	Map	..	..	..	Yes	2			1	
5382·3	3									
5370·5	1	..	Yes	4	Yes	10			3	
5369·0	3	..	Yes	4	Yes	1			1	
5366·5	3	..	..	..	Yes	1			1	
5364·0	3	..	..	..	Yes	1			1	
5361·9	4	..	..	..	Yes	20			10	
5352·4	4	Co	Yes	2	Yes	4			2	
5348·6	4									
5340·2	2	Mn	Yes	2	Yes	1			2	
5339·2	2	..	Yes	2						
5327·6	Map	..	..	..	Yes	5			2	
5327·3	1	..	Yes	2						
5327·1	Map	..	..	..	Yes	5			2	
5323·4	2									

Iron—*continued.*

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
5315·9	2	..	Yes	Distinctly weakened and sometimes reversed	Yes	90	50	
5306·5	3							
5301·5	3							
5282·6	2							
5280·9	3							
5269·5	1	..	Yes	3	Yes	15	4	Thalén gives Ca line at 5269·4.
5268·5	1	..	Yes	3	Yes	12	3	
5265·8	2	Co	Yes	2	Yes	10	4	
5262·4	4							
5254·1	Map	..	..	..	Yes	1	2	
5249·7	Map	..	..	..	Yes	3	1	
5246·3	Map	..	..	..	Yes	3	1	
5239·0	Map	..	..	..	Yes	4	2	
5232·1	1	..	Yes	2	Yes	1	3	
5229·0	Map	..	Yes	2				
5226·2	1	..	Yes	2	Yes	10	3	
5216·5	Map	..	..	..	Yes	2	1	
5215·5	Map	..	..	..	Yes	3	2	

5214·4	Map	..	..	..	Yes	2	1	
5207·6	3	Cr	Yes	4	Yes	10	6	
5203·7	3	Cr	Yes	4	Yes	10	6	
5201·5	4	..	Yes	2	Yes	5	3	
5197·9	Map	..	Yes	3	Yes	1	1	
5194·1	3	..	..	..	Yes	2	2	
5191·7	2	..	Yes	2				
5190·5	4	..	Yes	2				
5185·1	Map	..	..	..	Yes	5	2	
5171·7	4							
5168·3	3	Ni	Yes	4*	Yes	40	30	
5166·7	2	Mg	Yes	2*	Yes	30	20	Vogel gives a magnesium line at 516·7.
5161·6	4							
5151·3	Map	..	Yes	2				

Titanium.

6555·7	4							
6542·8	5							
6260·2	2	..	Yes	3 double				
6257·4	1							
6220·9	3	..	Yes	Bright	..	..	.	Brightness 2.
6218·3	Map	..	..	..	Yes	3	1	
6214·1	3	..	..	..	Yes	3	1	
6125·2	2							
6097·4	3							
6090·4	2							
6083·2	3							
6083·1	Map	..	Yes	3	Yes	3	2	
6064·5	2	Fe	Yes	3	Yes	5	2	Vogel gives an iron line, much thickened, at 606·4.
5998·7	2							

* But sometimes thinned, and even reversed.

Titanium—*continued*.

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
5978·0	1							
5977·1	Map	..	Yes	4				
5965·3	1	..	Yes	2				
5951·8	1							
5921·5	3							
5918·9	3							
5893·0	1							
5865·3	1	..	Yes	2				
5738·0	3							
5714·0	4							
5701·5	5							
5688·5	2							
5679·0	3							
5674·4	1							
5661·5	1	Fe	Yes	4	Yes	15	2	Vogel gives an iron line at 566·1.
5647·0	4							
5643·0	1							
5629·0	5							
5597·2	5							
5564·6	3							
5513·4	1	..	Yes	3				

5511·8	1							
5502·8	2							
5488·9	2		Yes	2	Yes	2	1	
5486·8	3							
5480·2	2	..						
5476·5	3							
5473·3	3							
5470·5	4							
5448·0	3							
5445·9	Map	..	Yes	4	Yes	10	4	Iron in tables.
5445·8	4							
5428·8	Map	..	Yes	3	Yes	8	3	Iron in tables.
5428·6	2							
5425·0	3	Ba	Yes	4	Yes	25	6	
5424·5	Map	..	..	..	Yes	5	2	
5417·9	4	..	Yes	2	Yes			
5408·6	2							
5403·1	3	Fe	Yes	4	Yes	5	3	
5396·1	2	Fe	Yes	7	Yes	4	2	
5380·2	3	..	..	..	Yes	3	2	
5368·8	2							
5350·5	2							
5336·8	1							
5335·9	Map	..	..	..	Yes	5	2	
5298·5	3							
5296·7	1							
5295·5	3							
5287·8	4							
5282·8	1							
5271·5	4							
5267·2	4							
5265·0	2							
5262·9	4							
5259·6	4							
5255·0	4							

Titanium—*continued*.

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
5251·0	4							
5246·3	2							
5238·5	2							
5226·0	3							
5224·3	Map	..	..	..	Yes	3	2	
5223·0	1							
5217·5	4							
5209·5	1	..	Yes	4	Yes	1	2	
5205·5	3							
5200·5	3							
5192·3	1							
5188·3	2							
5187·4	Map	..	Yes	3				
5187·3	Map	..	..	..	Yes	1	1	
5185·1	3	..	..	..	Yes	5	2	
5173·0	2							
5153·2	3							
5151·2	2							
5147·0	3							
5144·5	2							

5409·0	2	..	..	Chromium.	Yes	2	2		
5342·5	5								
5341·0	5								
5318·0	5	..	..		Yes	1	1		
5313·0	5								
5296·6	5								
5296·1	5								
5274·3	4								
5263·4	4								
5254·1	4								
5246·3	4								
5207·6	1	Fe	Yes	4	Yes	10	6		
5205·2	1	..	Yes	4	Yes	10	6		
5203·7	1	..	Yes	4	Yes	10	6		
6466·0	3	..	..	Cadmium.	..	..	..		
6438·1	Map								
6438·0	1								
6056·5	5								
6003·5	5								
5957·5	5								
5913·0	5								
5790·0	5								
5687·0	4								
5489·0	5								
5471·0	4								
5378·0	1								
5335·7	1								
5304·5	5								
5153·0	4								

Manganese.

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
6020·7	1							
6015·9	1							
6012·5	1							
5515·6	5							
5443·0	5							
5419·5	3							
5412·4	3	..	..	..	Yes	4	2	
5406·5	5	..	Yes	4				
5399·6	4	..	..	..	Yes	2	1	
5393·5	4							
5376·6	3							
5359·0	4							
5340·2	3	Fe	Yes	2	Yes	1	2	
5254·1	4	..	Yes	2	Yes	1	2	
5233·6	4	..	..	..	Yes	10	8	
5195·2	4	..	..	..	Yes	1	1	
Cobalt.								
6142·5	3							
6121·2	3	Ca	Yes	4	Yes	5	3	Vogel gives Ca line, much thickened, at 612·1.
6003·5	2							

5482·4	4							
5452·0	3							
5443·0	3							
5368·0	3							
5362·5	5							
5359·5	5							
5352·4	3	Fe	Yes	2	Yes	4	2	
5351·2	3							
5342·6	5							
5342·1	5							
5279·6	3 double							
5267·2	5							
5265·8	3 double	Fe	Yes	2	Yes	10	4	
5234·4	5							
5230·0	5							
5212·0	5							

Nickel.

6175·7	3							
6115·3	4	..	Yes	3				
6107·5	4							
5892·0	1							
5856·5	4	Ca	Yes	2	..	..	..	Vogel gives a calcium line at 585·6.
5475·9	3	..	..	..	Yes	1	1	
5175·6	5							
5168·3	5	Fe	Yes	4*	Yes	40	30	
5155·1	5							
5145·7	5							
5142·0	5							
5136·8	5							

* But sometimes thinned, and even reversed.

Strontium.

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
6550·4	4							
6501·5	2							
6407·0	1	..	..	..	..	..	..	Seen by Vogel.*
6387·0	3							
6380·0	4							
5970·5	5							
5850·0	5							
5540·0	3							
5533·5	2							
5522·5	2							
5503·5	2							
5485·0	3							
5480·0	1							
5256·0	2							
5238·5	1							
5228·5	3							
5225·3	3							
5223·5	3							

* The solar line here referred to is marked by Thalén in his map as due to iron, although in his tables he makes no mention of it. Kirchoff gives the same solar line coincident with strontium, the wave-length of which, according to Thalén, is 6407, whilst the solar line is nearer 6407·2.

Calcium.

6498·0	2							
6492·1	1	..	Yes	3				
6498·5	2							
6461·7	1	..	..	..	Yes	5	2	Vogel gives a Ca line at 646·2.
6449·0	2	Ba	Yes	2	..	..	..	Seen by Vogel.
6438·1	1	..	Yes	3	Yes	5	2	
6168·3	2	..	Yes	4 double	Yes	3	1	
6161·2	1	..	Yes	4	Yes	8	3	Vogel gives a Ca line at 616·1, very much thickened.
6121·2	1	Co	Yes	4	Yes	5	3	Vogel gives a Ca line at 612·1, much thickened.
6101·7	2	..	Yes	4	Yes	3	2	Vogel gives a Ca line at 610·1, much thickened.
5856·5	3	Ni	Yes	2	..	..	..	Vogel gives a Ca line at 585·6.
5601·7	4	Fe	Yes	2				
5600·2	3							
5597·2	3	Fe	Yes	2				
5593·4	2	..	Yes	2				
5589·0	4							
5587·6	1	..	..	..	Yes	2	2	
5580·8	4							
5348·6	2	..	Yes	3				
5269·5	Map	..	Yes	3	Yes	15	4	Given as iron line in Thalén's list.
5269·4	2							
5264·5	3	..	Yes	2				
5263·4	4							
5263·3	Map	..	..	..	Yes	1		
5261·2	5							
5260·8	5							
5188·2	3	..	Yes	3	Yes	10	5	

Barium.

Thalén.			Young.					Remarks.
			Spots.		Prominences.			
Wave-lengths of lines.	Intensities.	Coincident with	If visible.	Widening.	If visible.	How many times.	Brightness.	
6526·6	3							
6496·0	1	..	..	..	Yes	18	5	
6483·0	3							
6449·0	3	Ca	Yes	2	..	..	..	Seen by Vogel.
6343·0	3							
6140·6	1	..	..	..	Yes	25	10	
6109·9	3	..	..	..	Yes	2	1	
6062·0	3							
6018·0	3	..	..	..	Yes	2	1	
5991·5	3							
5971·0	3							
5904·5	5							
5852·7	Map	..	..	..	Yes	8	2	
5852·5	1							
5827·0	3							
5808·5	5							
5803·5	5							
5779·5	3							
5534·5	1							
5534·1	Map	..	Yes	2	Yes	50	12	
5521·5	3							
5518·7	Map	..	..	..	Yes	15	2	
5425·0	3	Ti	Yes	4				

Magnesium.

5527·4	1	..	Yes	2				
5183·0	1	..	Yes	3*	Yes	50	30	b_1 } Seen by Vogel.
5172·0	1	..	Yes	3*	Yes	50	35	b_2 }
5166·7	1	Fe	Yes	2	Yes	30	20	b_4. Vogel marks this line at 516·7.

* But sometimes thinned, and even reversed.

Sodium.

6160·0	2	..	Yes	4				
6154·2	2	..	Yes	4				
5895·0	1	..	Yes	6*	Yes	50	30	D_2 } Seen by Vogel.
5889·0	1	..	Yes	6*	Yes	50	30	D_1 }
5687·2	3	..	Yes	3	Yes	2	1	} Seen by Vogel.
5681·4	3	Fe	Yes	3	Yes	2	1	}
5154·8	3	..	..	..	Yes	3	1	
5152·5	3	..	..	..	Yes	3	1	

* But sometimes reversed.

XXVII. "On a New Method of Studying Metallic Vapours."

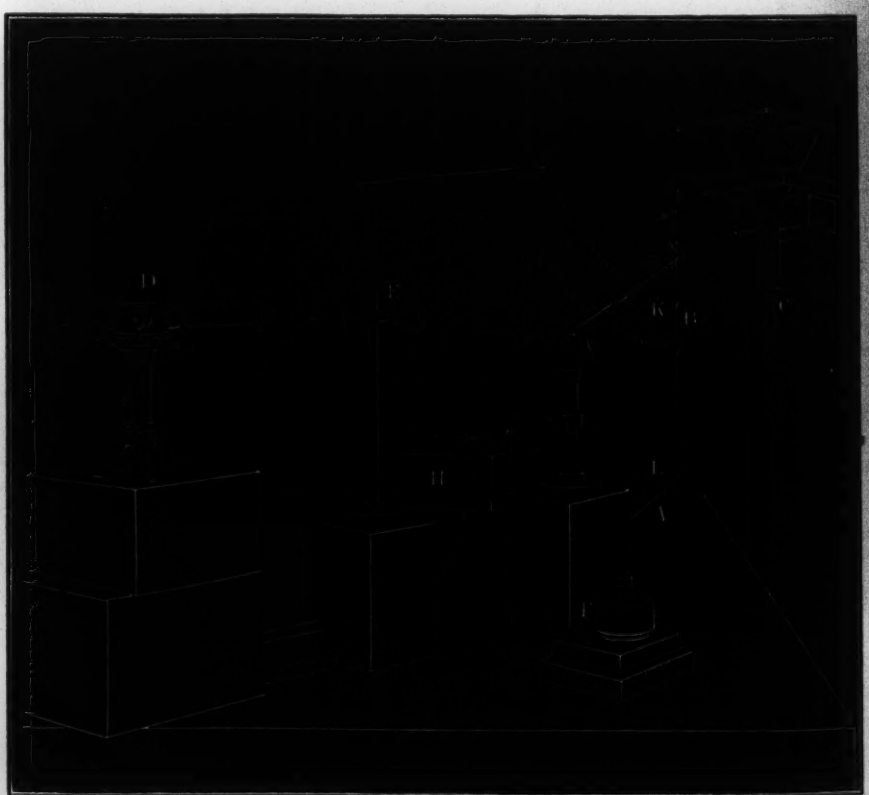
By J. Norman Lockyer, F.R.S. Received June 19, 1879.

On May 29 I read a note before the Royal Society, stating that I had lately been engaged in studying the spectrum of sodium under new experimental conditions, and I pointed out that the vapour given off from the metal after slow distillation in a vacuum for some time shows the red and green lines without any trace whatever of the yellow one.

No detailed account of the method employed in the experiments was given, as the most convenient form of apparatus had not then been determined.

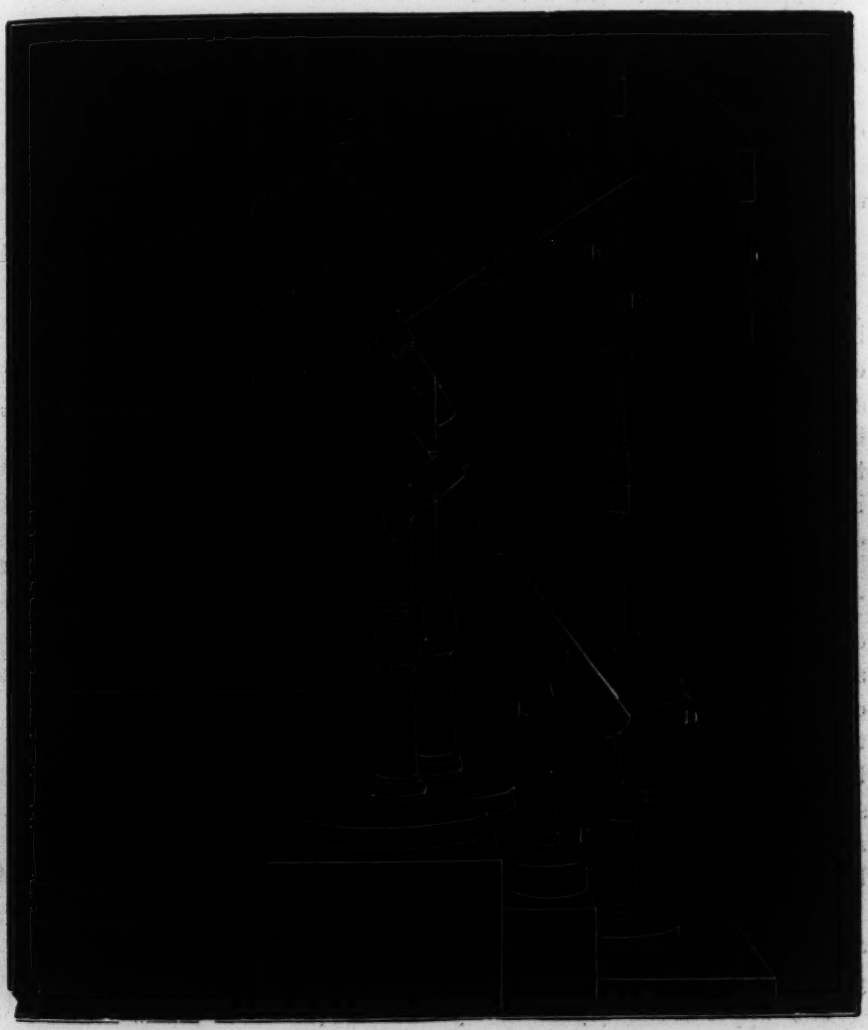
The novelty of the method consists in the use of the luminous electric current as an explorer and not as agent for the supply of the vapours under examination; that is to say, the vapours are first produced by an external source of heat, and are then rendered luminous by the passage of the current. The length and bore of the tube therefore control the phenomena to a certain extent.

A form of apparatus which I have found to answer very well is shown in the accompanying photograph.



A is the tube or retort containing the metal experimented on at its lower extremity, and having a platinum wire sealed into it at a distance of about two inches from the lower end, the other end being drawn out and connected by a mercury joint to an ordinary Geissler tube, which is connected by another mercury joint to the Sprengel pump C.

Another form of tube which I have used is prepared by inserting two platinum poles into a piece of combustion tubing sealed at one end, and after inserting the metal to be experimented on, drawing out the glass between the platinum to a capillary tube.



I have also tried inserting the platinum pole at the end of the retort so that the spark passes from the surface of the metal, but this arrangement did not answer at all.

Some other modifications have been tried, but the first form I have

described is that which I have found to answer best, so far as the trials have yet gone.

D is the spectroscope.

E is the lens used for focussing the image of the Geissler tube on the slit.

F is the spirit lamp for heating the retort.

H is the battery.

K and L are the wires connected with the coil.

In the second photograph the method of observing the spectrum of the vapours close to the surface of the metal is indicated; the same letters apply, D′ being however in this case a direct-vision spectroscope, which was sometimes employed for convenience.

For determining the exact positions of the lines in the spectrum of the vapour in any part of the retort, the larger spectroscope, with its illuminated scale, was used in the place of the direct-vision spectroscope.

The secondary wires of the coil were connected, one with the pole in the upper bulb at B, and the other with the platinum at A.

B is an ordinary Geissler tube with two bulbs separated by a capillary tube. The great advantage of this arrangement is that this capillary portion can be used for ascertaining what gases or vapours are carried over by the pump without any interference with the retort, both wires being connected with the Geissler tube. If, for example, the sodium contains an impurity of hydrocarbon, the moment at which it begins or ceases to come off can be found by examining the spectrum of this capillary tube.

After a vacuum has been obtained the retort is heated gradually. The pump almost immediately stops clicking, and in a short time becomes nearly full of hydrogen. The spectrum of the capillary then shows the hydrogen lines intensely bright. After some time the gas comes off far less freely, and an approach to a vacuum is again obtained. Another phenomenon now begins to show itself: on passing the current a yellow glow is seen, which gradually fills the whole space between the pole in the retort and the metal; its spectrum consists of hydrogen and the yellow line of sodium, the red and green Na line being both absent until the experiment has gone on for some time.

As the distillation goes on, the yellow glow increases in brilliancy, and extends to a greater distance above the pole, and the red and green Na lines presently make their appearance as very faint lines.

The upper boundary of the yellow is quite sharp, the lines and "structure" spectrum of hydrogen appearing above it.

After the yellow glow-giving vapour (which does not attack the glass) has been visible for some time, the pump is stopped and the metal heated more strongly. On passing the current a little while afterwards, a very brilliant leaf-green vapour is seen underlying

the yellow one, and connected with it by a sap-green vapour. The spectra then visible in the tube at the same time are—

Leaf-green.... Green and red lines of Na and C of hydrogen; D absent.

Sap-green..... Green, red, and yellow Na lines of equal brilliancy and C of hydrogen.

Yellow ..... D alone and C.

Bluish-green .. C and F and hydrogen structure.

To observe the green Na line alone it is necessary to point the direct-vision spectroscope just above the surface of the metal where the green is strongest. It is also necessary to guard against internal reflections from the glass, as this may sometimes cause the D line to be seen by reflection from the surface.

The metallic sodium is apt to condense on the surface of the glass and around the platinum pole. If this should happen the retort must be heated until the sodium has again volatilised. The yellow line is always seen when the coating extends to the pole.

The following extracts from the laboratory note-book will help to make the method better understood :—

Experiment with sodium.

Before heating Na, the following lines were visible in Geissler tube.

8·8 to 8·9=C.
9·72 much brighter than other lines in hydrogen structure.
10·6
10·95=Hg green.
12·7
12·85=F.
13·85
15·3 to 15·4 =Hg violet.

Colour of spark along tube, pale lilac; its spectrum exhibited—
Hydrogen structure.
C.

No hydrocarbon spectrum visible.

Time 11.25.—Na gently heated with spirit-lamp four inches away.

Almost linear hydrogen spectrum in capillary structure gone or nearly so.

Hydrocarbon now strong below lower mercury joint.

Time 11.35.—Sodium still angular.

Hydrocarbon scarcely visible in capillary.

Na again warmed.

11·7 (hydrocarbon line) increased in narrow tube below lower mercury joint, showing that hydrocarbon still comes off.

10·15 (mercury yellow) bright.

Heating continued; pump stopped clicking.

Time 11.37.—Na melted; spark refuses to pass; glass instantly covered with white deposit where the sodium boiled.

Spark now passes, showing stratification.

Spectrum of retort consisted of pure hydrogen without a trace of D; all traces of hydrocarbon also gone. Hydrogen line G seen very bright.

Pump clicking.

Na again heated, not boiled; pump stopped clicking.

Yellow glow in retort showed D very broad, C without F.

Time 11.50.—Pump nearly clicking; stratifications in retort; spark bright red in capillary, pure hydrogen with blue line remarkably brilliant; D visible two inches above pole in retort; yellowish film forming at top of retort five inches from sodium; retort otherwise clean; pump not clicking, much hydrogen being carried over.

Structure spectrum in retort getting much brighter; discoloration at top removed by Bunsen, but forms a beautiful green film below; glass just above Na clean; trace of D three inches from pole.

Time 11.55.—Still pure hydrogen in capillary.

Metal slowly distilling and film forming above it; very bright yellow glow at pole in retort, gives no trace of the green line; much hydrogen coming over.

Time 12.0.—Pump now clicking; only hydrogen visible in capillary; yellow glow now seen over metal. Glass cleared by Bunsen has been discoloured by platinum pole; on heating deposit, D seen very bright, C scarcely visible, and very faint trace of green line; D visible for three inches above pole, gradually passing away into hydrogen structure; pump carrying over much hydrogen, which is brilliant in capillary.

Pump beginning to click, metal more strongly heated; clicking stopped; new system of stratifications in retort, glowing with a blue light as far as mercury joint; violet above mercury joint. On further heating, the old conditions are restored. In the retort, D now bright one inch above pole; C very dim; no green line.

Na boiled; spark refuses to pass; spirit lamp raised one inch; spark again blue in tube; its spectrum consists of hydrogen structure without F or C. Tube again cleared by Bunsen flame; Na boiling; yellow glow blending with violet one inch from pole; fine stratifications in higher part of retort; pump carrying over much hydrogen.

Pump clicking; Hg line at 11·0 visible; violet Hg line very faint.

Tube again cleared by Bunsen flame; yellow line two inches from pole; green line very faint; no trace of C; pump carrying over hydrogen; violet light now visible over metal.

Time 12.20.—Tube again cleared with Bunsen; olive-green now becoming apparent at pole; green line well seen; tube now keeps

quite clear, although position of Bunsen is unchanged; pump nearly clicking; hydrogen very bright in retort. New spectrum visible in capillary.

Pump clicking; glass becoming attacked; blue film one inch above pole in retort; glow extends back to metal; line seen intermediate in position between D and Na green.

Red hydrogen very intense in capillary tube; blue line brighter than ever; fluffy line in green (11·4?).

Olive-green glow in retort; vapour becoming greener; green line as bright as D; D now fainter than either the green or red Na lines; the red line gradually fades on cooling; only one line more refrangible than the green line. The green vapour does not appear to attack the glass; D now on point of disappearing; green line far brighter.

The tube then cracked and put an end to the experiment.

This experiment, although not successful in so far as the isolation of the green Na line is concerned, is brought forward, as it shows the details of the observations as dictated by me to my assistant while the experiment was going on. The experiment has since been successfully repeated several times, simply with the object of getting the green line alone. This is best achieved by heating the metal before all the hydrogen has been extracted by the pump.*

The method has been tried also with potassium, calcium, and some other metals, and with metallic salts. I do not, however, yet bring forward the results I have obtained, as the experiments are beset with difficulties; and I am now carrying out a research with the hope of eliminating all sources of error.

I may state, however, that with potassium and calcium I get the same inversion of phenomena, the yellow-green lines of potassium being seen without the red; while in the case of calcium the blue line alone was seen.

The fact that in these experiments we get, as before mentioned, vapours which at one and the same time exhibit different colours and different spectra at different levels in the tube, at once suggests the phenomena of fractional distillation, and supports the following statement which I made last March:—

"Doublets and triplets, as a rule, reverse themselves more freely than the irregular lines in the same spectrum; which particular doublet or triplet will reverse depending upon the temperature, as if the cooler vapour to which the reversal is due varied, as in the case of fractional distillation."

It is obvious that if, at any particular temperature, one particular kind of vapour is given off in the manner indicated, in excess of all

* In this extract from the laboratory note-book I have left out in most cases lists of lines, as I have not as yet been able to assign to all of them their wave-lengths.

others or alone, then we shall get an excess in the reversal of the lines special to that particular kind of vapour given off.

It is remarkable from this point of view that I have only succeeded in constantly reversing a triplet in the case of manganese, and that I have only succeeded in reversing a triplet of iron on one or two occasions only. Comparing these results with those obtained in the case of Na, K, Ca, &c., we have here an indication of a considerable physical difference between the metals of the alkalies, for instance, and those of the iron group.

It is also suggested as a result of the application of this new method of experimentation, that in the case of a considerable number of chemical substances not only the line spectrum is compound in its origin, as I suggested many years ago, but that a large number of the lines is due to molecular groupings of considerable complexity, which can be kept out of the reaction by careful low temperature distillation.

Whether these higher molecular groupings are merely larger physical aggregates or whether they be compounds in a stricter sense, hydrides for instance, it may take some time to determine; but it is obvious that however this may be, the finer vapours may exist uncontaminated with the grosser ones in the incandescent atmosphere of a star; and I know of no reason why the molecules of such fine vapours should not combine with each other, and if, for the sake of argument, we assume that some of the so-called chemical elements may be thus compounded of those finer vapours which we know to exist in the atmospheres of the hottest stars, then we may expect to find the high temperature spectra of these so-called elements to consist of a complication of lines, due partly to the complex molecules themselves and partly to the finer vapours in question.

XXVIII. "Supplementary Note on the Volumetric Estimation of Sugar by an Ammoniated Cupric Test giving Reduction without Precipitation." By F. W. Pavy, M.D., F.R.S. Received June 19, 1879.

In a communication published in the "Proceedings of the Royal Society" (vol. xxviii, p. 260), I drew attention to the employment of an ammoniated cupric liquid for the volumetric estimation of sugar. Instead of giving an altogether new formula for this liquid, I recommended it to be prepared from Fehling's solution with the addition of a certain volume of ammonia and of water. Under the method of analytical procedure adopted in my laboratory, which consisted in running in the liquid pretty quickly from the burette to near the required point, and then dropping it in slowly, the amount of soda contained in Fehling's solution proved sufficient to give accurate

results. I have since learnt, however, that if the liquid to be examined be dropped in slowly throughout, by which a longer time is occupied in the performance of the operation, the result becomes to a certain extent modified. It appears as though the test under the influence of prolonged ebullition fails to possess the requisite stability. In the case of Fehling's solution used in an ordinary way I have long known that when in a diluted state, ebullition alone leads to the occurrence of change, unless an additional amount of alkali be introduced. A certain strength of alkalinity is required to give the requisite stability to resist the effect of ebullition. With the ammoniated liquid, it is to be remembered, a state of considerable dilution exists, viz., to the extent of 120 in 1,000.

The introduction of a larger amount of fixed alkali into the ammoniated liquid suffices to give it the power to resist the influence of the variation in the method of procedure referred to in conducting the analysis, and it is to make known the fact of the alteration being required, that this communication is presented.

If Fehling's solution be used in the preparation of the ammoniated liquid, as I originally suggested, the amount of soda employed should be doubled to meet the requirements.

It was on account of Fehling's solution being so well known a liquid, that I recommended the ammoniated test to be made from it. Now that a modification has to be introduced, I may state that the copper liquid I have generally been in the habit of using as a sugar test is made with potash instead of soda, and contains a much larger proportion of alkali than what is present in Fehling's solution. It has appeared to me that the liquid thus constituted possesses the advantage of greater precision of behaviour, and it may be used for the preparation of the ammoniated test. The following is the composition recommended:—

Cupric sulphate	34·65	grms.
Potassic sodic tartrate (Rochelle salt) ..	170	„
Potash	170	„
Water to 1 litre.		

120 cub. centims. of this liquid are mixed with 300 of strong ammonia (sp. gr. 0·880) and water added to a litre for the ammoniated test, 20 cub. centims. of which stand equivalent to 0·010 grm. of glucose.

XXIX. "About some General Laws of Animal Life." By Nicholas Wagner, Professor of Zoology, University, St. Petersburg. Cómmunicated by Professor Huxley, Sec.R.S.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 20th.

Presents, June 19, 1879.

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Cambridge (Mass.):—Museum of Comparative Zoölogy. Bulletin. Vol. V. No. 10. 8vo. 1879. The Museum.

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Halle:—Naturforschende Gesellschaft. Abhandlungen. Band XIV. Heft 3. 4to. 1879. The Society.

Leipzig:—Astronomische Gesellschaft. Vierteljahrsschrift. Jahrg. 14. Heft 2. 8vo. 1879. The Society.

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London Mathematical Society. Proceedings. Nos. 141, 142. 8vo. 1878. The Society.

Meteorological Society. Quarterly Journal. Vol. V. Nos. 29, 30. 8vo. 1879. List of Fellows, 1879. 8vo. The Society.

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Photographic Society. Journal and Transactions. Vol. III. Nos. 1–8. 8vo. List of Members. 1879. The Society.

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Royal Microscopical Society. Journal. Vol. II. No. 4. 8vo. 1879. The Society.

Royal United Service Institution. Journal. Vol. XXIII. No. 99. 8vo. 1879. The Institution.

Society of Antiquaries. Proceedings. Vol. VII. No. 5. 8vo. *London.* The Society.

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R. Accademia de Lincei. Serie Terza. Transunti. Vol. III. Fasc. 3–6. 4to. 1879. The Academy.

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Dublin:—Weekly Returns of Births and Deaths. Vol. XV, No. 24, to Vol. XVI, No. 23. Quarterly Return. Nos. 57–61. The Registrar-General for Ireland.

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American Journal of Otology. Vol. I. No. 2. 8vo. *New York* 1879. The Editor.

American Journal of Science and Arts. Nos. 99–102. 8vo. *New Haven* 1879. The Editors.

Analyst. Nos. 28–39. 8vo. *London* 1878–79. The Editors.

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Educational Times. July to December, 1878. January to June, 1879. 4to. *London.* The Editor.

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Notes and Queries. July to December, 1878. January to June, 1879. 4to. *London.* The Editor.

Observatory. Nos. 15–26. 8vo. *London* 1878–79. The Editor.

Philosophical Magazine. July to December, 1878. January to June, 1879. 8vo. *London.* W. Francis.

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McLachlan (Robert). A Monographic Revision and Synopsis of the Trichoptera of the European Fauna. Part 8. 8vo. *London* 1879. The Author.

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Moon (Robert). On some points in the theory of the Infinite and of Infinitesimals. 8vo. *London* 1879. The Author.

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Wilkins (N. G.) Rules for effecting certain operations in Division. 8vo. *Hanover* 1878. The Author.

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November 20, 1879.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Mr. George Matthey was admitted into the Society.

Mr. Bramwell, Mr. Busk, Mr. De La Rue, Mr. Newmarch, and Mr. Perkin, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Experimental Researches on the Electric Discharge with the Chloride of Silver Battery." By WARREN DE LA RUE, M.A., D.C.L., F.R.S., and HUGO W. MÜLLER, Ph.D., F.R.S. Received August 7, 1879.

(Abstract.)

PART III.—*Tube-Potential; Potential at a Constant Distance and Various Pressures; Nature and Phenomena of the Electric Arc.*

In the first part of this paper the authors describe a series of experiments to determine the potential necessary to produce a discharge in a tube, exhausted gradually more and more while using a constant number of cells in all the experiments. In consequence of the life of the battery becoming so much exhausted by the method employed, the experiments were confined to one gaseous medium, namely, hydrogen. Since the completion, however, of the measurements described in the paper, the authors have found two other more convenient methods for determining the tube-potential, which do not exhaust the battery injuriously; these are described in an appendix. The tube, 162, employed was 33 inches long and 2 inches in diameter, the distance between the ring and straight wire terminals being 29·75 inches; the battery consisted of 11,000 cells. The discharge took place when the pressure was reduced to 35·5 m.m., 46,710 **M**, and the exhaustion was afterwards continued gradually until it fell to 0·0065 m.m., 8·6 **M**. In commencing each set of experiments the deflection of a tangent-galvanometer was observed when the battery was short-circuited. By

a table previously calculated, the value of the deflection in ohms of resistance per cell could be read off; this multiplied by 11,000 gave the total resistance of the battery; the tube was then connected with the terminals and the galvanometer again observed; this gave a less deflection and indicated a greater resistance, which, multiplied by 11,000, gave the total resistance of the tube and battery: by subtracting the resistance of the battery the resistance of the tube was ascertained. Calling the total resistance R, the tube resistance r, the tube-potential V, $V=\frac{r\times 11{,}000}{R}$. The tube-potential requisite to produce a discharge, with a pressure of 46,710 **M**, was found to be 10,250 cells; this gradually fell until a pressure of 0·642 m.m., 1,082 **M**, was reached, the tube-potential being then only 430 cells, after which it rapidly rose and, at 8·6 **M**, it required a potential of 8,937 cells to produce a discharge. From the experiments described in Part II (p. 103, foot note), it was found that, in another tube, it required the full potential of 11,000 cells to produce a discharge at 3 **M**, and that, at 1·8 **M**, this potential was insufficient. The obstruction to the discharge in tube 162 was as great at 8·6 **M**, as at 28,553 **M** pressure, and required 8,950 cells in each case. A few of the more remarkable phases of stratification in tube 162 are shown in one of the plates which illustrate the paper.

The next part of the paper deals with the potential necessary to produce a discharge between disks 1·5 inch in diameter, at a constant distance and at various pressures.

In the first instance, an experiment was made in order to ascertain whether there was either any condensation or dilatation of the gas in contiguity with the terminals before the actual passage of the discharge. In order to do this an apparatus was constructed, as shown in fig. 1.

It consists of a glass cylinder, the depth of which is accurately the same in every part, so as to ensure the parallelism of two glass disks which close its ends.

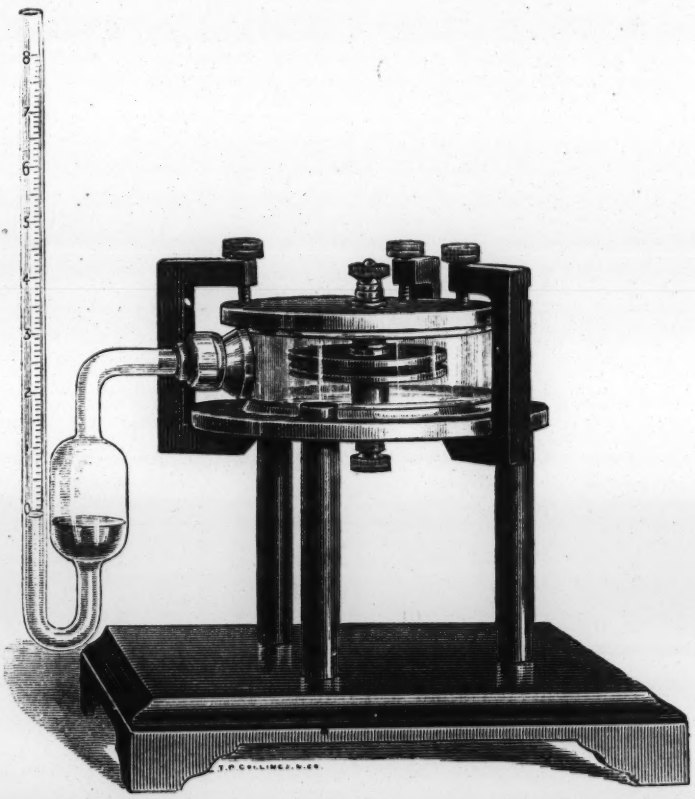
These are held in contact with the ends of the cylinder by means of screw-clamps made of ebonite, and the whole apparatus is supported on a tripod ebonite stand, which is fastened to a square wooden foot. Attached parallel to the top and bottom glass disks, by means of flanged-screw rods, are two brass disks with rounded edges, 4 inches in diameter; these are maintained at a distance of 0·13 inch, 3·3 m.m., at which the discharge of 11,000 cells would only just take place.

The ends which project through the glass disks are furnished with binding-screws for attaching wires from the battery.

On the side of the cylinder is a tubulure in which is fitted a gauge containing strong sulphuric acid, so as to dry the inside of the apparatus, and to indicate whether any condensation or dilatation of the

gas contained in the cylinder occurs on connecting the metallic disks with the battery by means of the contact-key. The edges of the cylinder were rubbed with grease, and care was taken to prove that the apparatus was perfectly tight, by causing the fluid in the limb of the gauge to stand for some time higher than that in the bulb. When contact was made with a battery of 9,800 cells, there was not the

Fig. 1.



slightest indication of any alteration of volume of the contained air, so that there was neither condensation about the disks which would have caused a contraction, nor repulsion from the disks which would have caused an expansion of volume. The fluid in the stem was observed with a lens, but not the slightest motion of it took place. The same result was noticed even when water was substituted for sulphuric acid. So far, then, as this apparatus would indicate it, the result is entirely negative.

Potential necessary to produce a discharge between disks at a constant distance and at various pressures.

The experiments were made by placing the micrometer-discharger, Part I, fig. 1, under a bell-jar, and in the first instance adjusting the

disks to the striking distance at atmospheric pressure for the battery of 11,000 cells. Afterwards a less number of cells was connected with the disks and the bell-jar gradually exhausted until the discharge occurred; the height of the gauge was then read off. Then a less and less number of cells was connected with the disks and the operation was repeated. There was a gauge about 36 inches long attached to the pump-plate in order to indicate the pressure beyond the range of the gauges attached to the pumps.

In air the discharge took place at ordinary atmospheric pressure with 11,000 cells when the disks were 0·13 inch, 3·3 m.m. distant; and with 600 cells at an average pressure of 10 m.m.

In hydrogen it took place at atmospheric pressure with 11,000 cells when the disks were 0·22 inch, 5·59 m.m. distant; and with 600 cells at an average of 14 m.m. pressure.

In carbonic acid, at atmospheric pressure with 11,000 cells, when the disks were 0·122 inch, 3·096 m.m. distant; and with 600 cells at an average pressure of 5·2 m.m.

The numbers obtained for air, hydrogen, and carbonic acid respectively were plotted down on millimetre scale paper, the abscissæ being 1 m.m.=2,500 **M**, the ordinates 1 m.m.=25 cells, and curves drawn to give a mean of the several observations. These appeared to resemble hyperbolic curves so closely that true hyperbolic curves were found partly by a geometric construction, partly by computation, which would intersect the mean experimental curves in two points. The results of experiment were again laid down on these new curves, and it was found that they did not differ more from them than they did from each other.

The ratio of the transverse axis (pressure) to the conjugate axis (potential) of the hyperbolas set out on the above-mentioned scale, was—

For air	0·9665
,, hydrogen	1·0170
,, carbonic acid	1·0690

The striking distances at atmospheric pressure for spherical surfaces 3 inches radius and 1·5 inch diameter, with various potentials, as given in Part I, page 14, curve VIII, and at page 64, also those for nearly flat surfaces in pages 19 and 64, were reduced to millimetres distance and plotted down in the same way, but not on precisely the same scale as the preceding curves for constant distance and various pressures. Hyperbolic curves were also found which intersected the experimental curves in two points.

It was seen in the case of spherical surfaces, the result having been obtained as the average of a great number of experiments, that the hyperbola coincided closely with the observations, while for plane

surfaces, for which only a few experiments were made, the coincidences were not quite so perfect. Nevertheless, it would appear that the law of the hyperbola holds equally well for a constant pressure and varying distance as it does for a constant distance and varying pressure; the obstacle in the way of a discharge being up to a certain point as the number of molecules intervening between the terminals.*

In the two cases of spherical and plane surfaces the ratio between the transverse (distance) and conjugate (potential) axes of the respective hyperbolas was—

For spherical surfaces	1·240
„ disks	1·285

With the data already published in Part I, the authors have laid down a fresh curve for the striking distance between flat disks on a scale of 10 centims. for a millimetre and 5 centims. to 1,000 cells.

From the curve thus laid down the following numbers were deduced:—

EMF in volts.	Striking distance in centimetres.	Difference of potential per centimetre.	Intensity of force.	
			Electromagnetic.	Electrostatic.
		volts.		
1,000	0·0205	48,770	$4·88 \times 10^{12}$	163
2,000	0·0430	46,500	4·65 „	155
3,000	0·0660	45,450	4·55 „	152
4,000	0·0914	43,770	4·38 „	146
5,000	0·1176	42,510	4·25 „	142
6,000	0·1473	40,740	4·07 „	136
7,000	0·1800	38,890	3·89 „	130
8,000	0·2146	37,280	3·73 „	124
9,000	0·2495	36,070	3·61 „	120
10,000	0·2863	34,920	3·49 „	116
11,000	0·3245	33,900	3·39 „	113
11,309	0·3378	33,460	3·35 „	112

The remainder of the paper is chiefly occupied with the study of the phenomena of the electric arc under various conditions of distance, pressure, and potential; the results obtained support the view that the arc and the stratified discharge are merely modifications of the same phenomenon.

* Dr. Alexander Macfarlane has published in the "Transactions of the Royal Society of Edinburgh," 1878, vol. xxvii, an elaborate and careful research of the "Disruptive Discharge of Electricity" in air and different gases, and between terminals of various forms. An abstract of this paper will be found in "Nature," December 26, 1878, pp. 184, 185. Dr. Macfarlane used a Holtz machine and employed higher potentials than those we used; he found that the results for the discharge between two disks 4 inches in diameter at various distances up to 1·2 centims. and with various pressures were satisfactorily represented by the hyperbola.

The experiments were made in a bell-jar, containing the terminals, which could be gradually exhausted after having been filled with air or other gas. One of the terminals was fixed to the bottom plate, the other could be adjusted to any distance from it by a rod sliding through a stuffing-box in the glass cover. The foot of the stand was insulated by a disk of ebonite, on which it stands. One such bell-jar is $9\frac{1}{4}$ inches (23·4 centims.) high and $5\frac{7}{8}$ inches (14·9 centims.) in diameter; its cubical content, obtained by covering the open ends with glass plates and filling with water from a graduated measure, was found to be 3,787 cub. centims.

A remarkable phenomenon was observed on making connexion between the terminals and the battery by means of the discharging key (already described in Part I, page 4), namely, that within certain limits of pressure in the bell-jar a sudden expansion of the gas took place, and that as soon as the connexion was broken the gas then as suddenly returned nearly, but not quite, to its original volume in consequence of a slight increase of temperature. The effect was exactly like that which would have been produced if an empty bladder had been suspended between the terminals, and suddenly inflated and as suddenly emptied.

The followed experiment in rarefied air, at a pressure of 56 m.m., at a temperature of 17°·5 C., will give an idea of the amount of instantaneous expansion which occurs when the terminals are connected with the poles of the battery of 11,000 cells, current 0·01102 **W**; the resistance of the bell-jar was reproduced by substituting 600,000 ohms wire resistance.

	m.m.	M.
Distance of the terminals—the top one a point, the lower a disk—6 inches; pressure	56	73,684
On making contact the arc passed and the column of mercury was depressed.....	15·8	20,789
Pressure on connexion.....	71·8	= 94,473

The increased was to the normal pressure in the ratio of 1·282 to 1; as the gas was kept at a constant volume, and supposing the expansion to be due to an increase of temperature, the pressure would vary as the absolute temperature,* therefore $\frac{T'}{T}=\frac{71{\cdot}8}{56}=1{\cdot}282$, whence $T'=1{\cdot}282\times 291{\cdot}2=373°{\cdot}3$ C.; $(373{\cdot}3-273{\cdot}7)=99°{\cdot}6$ C., the temperature of the bell-jar, and $(99{\cdot}6-17{\cdot}5)=82°{\cdot}1$, the rise of temperature while the discharge was taking place. But the temperature of the bell-jar as determined by a thermometer enclosed in it with its bulb uppermost only rose 0°·64 C. per second, taking into account the rate

* Absolute zero = 273·7 C., 273·7 + 17·5 = 291·2.

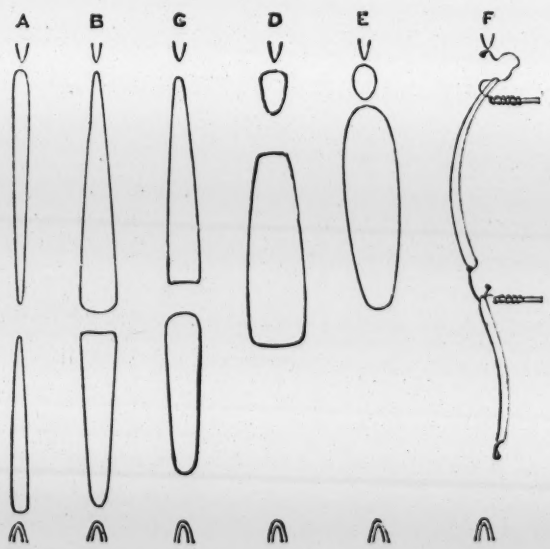
of cooling. It is evident, therefore, that the increase of pressure cannot be ascribed to the instantaneous heating of the bell-jar 82° C.

Taking the dimensions of the arc from a photograph, it was calculated that it must have attained the enormous temperature of 16,114° C., if the increase of pressure was really due to heat. It was found that platinum wires 0·001 inch in diameter supported in various parts of the arc were immediately fused; the temperature of the arc was therefore as high as the fusion-point of platinum, and possibly considerably higher.

If the whole of the heat evolved by a current of 0·01102 **W**, through a resistance of 600,000 ohms had been communicated to the air in the jar, weighing 0·339 grm. it would raise it 215°·6 C. in one second. It is known from direct experiment that this enormous evolution of heat was not communicated to any extent to the air in the bell-jar, because its temperature only increased about 0°·64 C. per second; the heat must consequently have escaped almost instantaneously by radiation. It is difficult consequently to realise the conjecture that the enormous dilatation which occurred instantaneously could have been caused by increase of temperature. And it points to its being produced by a projection of the molecules by electrification causing them to press outwards against the walls of the containing vessel, this pressure being distinct from the motion caused by heat.

The authors proceed to describe the appearance of the arc with terminals of various forms at different distances and with various pressures. It was found that the light emitted by different parts of the arc was not of the same intensity throughout, and that from the first there was a tendency to break up into distinct entities, as shown in the

Fig. 2.



diagram, fig. 2, which only indicates the central bright portion of the arc, this never quite reached the negative terminal, near which there was always the well-known dark discharge.

As the pressure was diminished the arc widened out until at last the entire surface of the negative disk was covered with a luminous halo, and the discharge took up a stratified appearance.

The appearances presented by the arc in air, hydrogen, and carbonic acid are copiously illustrated by copies, in mezzotint, of photographs and drawings as in Part II in the case of stratified discharges in tubes. During the course of these observations the increased pressure caused by the current was recorded, and an arrangement was made to ascertain whether the increase of pressure was greater near one or the other terminal. For this purpose a divided bell-jar was constructed, both ends of the two halves being accurately ground, and a glass disk divided the chamber into two parts; in this there is a hole half an inch in diameter made with a raised rim, in order that plates of mica with holes of different diameters might be cemented with Canada balsam centrally to the diaphragm. The capacity of the upper half was found to be 1,530 cub. centims., that of the lower half 1,755, total 3,285.

That portion of the chamber which was in communication with the gauge was connected alternately with the positive or negative terminal of the battery, and the depression noted. When this chamber was either − or + the ratio of increase to the normal pressure was—

−	+
1·44	1·39
1·34	1·41
1·13	1·20
1·22	1·34
1·38	1·40
1·16	1·11
1·19	1·16
1·266	1·287

It would appear therefore that the dilatation of the gas is the same both in the positive and negative chambers.

In order to prosecute their experiments in a vessel of still greater capacity, the authors had constructed a larger jar with a neck at each end, or more properly speaking perhaps, a tube supported horizontally on ebonite crutches. It is 37 inches long and $5\frac{13}{16}$ inches in diameter, its cubical content was found to be 14,435 cub. centims., or 3·8 times that of the bell-jar employed in the experiments on the electric arc.

The experiments with this tube will necessarily occupy a considerable period, partly on account of the long time it takes to exhaust

it after each set, partly on account of the variety of experiments it is intended to make with it; consequently they describe only a few of the first results hitherto obtained.

For Example in Air.

Pressure 3 m.m., 3,947 **M**, 6,300 cells. Two luminosities were formed, the ring negative being surrounded with a nebulosity which completely filled the end of the tube. The tube glowed brilliantly with a blue fluorescent light, which proved to have great actinic power. A dry-plate photograph obtained in 5 seconds records a very curious phenomenon, namely, that the outer boundary of the luminosity appears *darker* than the tube. It is to be remarked that while the discharge was reddish (nitrogen), the fluorescence of the tube was blue; the effect appears to be due to the absorption of a portion of the fluorescent light emanating from the back of the tube in passing through the red luminosity. The effect was quite unexpected, and it was thought at first that it might have arisen from some peculiarity in the development of the dry plate; it was not therefore until the result had been confirmed by other photographs that they ventured on the explanation above given.

The paper closes with the following conclusions:—

1. *For all gases there is a minimum pressure which offers the least resistance to the passage of an electric discharge. After the minimum has been reached, the resistance to a discharge rapidly increases as the pressure of the medium decreases.* With hydrogen the minimum is 0·64 m.m., 842 **M**; at 0·002 m.m., 3 **M**, it is as great as at 35 m.m., 46,000 **M**.

2. *There is neither condensation nor dilatation of a gaseous medium in contiguity with charged terminals.*

3. *When the discharge takes place there is a sudden dilatation of the medium in addition to and distinct from that caused by heat. This dilatation ceases instantaneously when the discharge ceases.*

4. *The potential necessary to produce a discharge between parallel flat surfaces at a constant distance and various pressures, or at a constant pressure and various distances, may be represented by hyperbolic curves. The resistance of the discharge between parallel flat surfaces being as the number of molecules intervening between them.*

5. *This law does not hold with regard to points. In Part I it has been shown that the potential necessary to produce a discharge at the atmospheric pressure and various distances is as the square root of the distances, while with a constant potential and various distances, the pressure has to be diminished in a greater ratio than that of the increase of distance in order to permit a discharge to take place.*

6. *The electric arc and the stratified discharge in vacuum tubes are modifications of the same phenomenon.*

Lastly, the authors say:—

"We have again pleasure in thanking Professor Stokes for his much-valued advice during the course of our investigations. To our assistant, Mr. Fram, we are indebted for his able co-operation, and we have to thank Mr. H. Reynolds for his aid and skill in taking photographs."

II. "Researches on the Action of Organic Substances on the Ultra-Violet Rays of the Spectrum. Part III. On Examination of Essential Oils." By W. N. HARTLEY, F.R.S.E., &c., Professor of Chemistry in the Royal College of Science for Ireland, Dublin, and A. K. HUNTINGTON, F.I.C., F.C.S., Associate of the Royal School of Mines. Received July 22, 1879.

(Abstract.)

Much chemical and physical research by various investigators has been devoted to the class of bodies known as Essential Oils, as, for instance, the work of Dr. J. H. Gladstone ("Journal of the Chemical Society," vol. xviii, p. 1; vol. xxiii, p. 147; vol. xxv, p. 1); of Dr. C. R. A. Wright ("Journal of the Chemical Society," vol. xxvi, pp. 549 and 686; vol. xxvii, pp. 1, 317, and 619, Isomeric terpenes and their derivatives); and of Dr. W. A. Tilden (*loc. cit.*, vol. xxviii, pp. 514 and 1258), as well as of many others.

The new method of research employed by us and described in a paper about to be published in the "Philosophical Transactions," has been applied to the examination of these substances. We have to acknowledge the kindness with which several gentlemen have supplied us with samples of essential oils, namely, Dr. Gladstone, Mr. Farries, of the firm of Burgoyne, Burbidges, Cyriax, and Farries, Dr. Septimus Piesse, and Dr. W. A. Tilden.

As in our previous experiments (Abstracts of Parts I and II, "Proc. Roy. Soc.," No. 192, 1879), photographs were taken of the spectrum transmitted by the undiluted liquid, and then of that transmitted by the liquid in various states of dilution, the dilutions ranging in some cases from 1 in 50 to 1 in 500,000 volumes of alcohol.

The following is a list of substances examined, classified according to the optical properties they were found to possess.

Oils and hydrocarbons transmitting continuous spectra.

Australene, from oil of turpentine.
Birch bark.
Cajputene dihydrate.
Carraway hydrocarbon (No. 2).
Calamus.
Citron.
Citronella.
Cedar wood.

Cedrat hydrocarbon.
Cubebs.
Elder.
Hesperidene, from oil of orange-peel.
Indian Geranium.
Juniper.
Lavender.
Lign-Aloes.
Melaleuca Ericifolia.
Menthol, from oil of mint.
Nutmeg hydrocarbon.
Patchouli, oil of, Nos. 1 and 2.
Rose, otto of.
Rosewood.
Rosemary.
Santal wood.
Terebene.
Terebenthene.
Vitivert.

Hydrocarbons showing the absorption bands of cymene.

Thyme.
Lemon.
Nutmeg.
Carraway (No. 1).

Substances showing strong bands of absorption in the spectrum transmitted by dilute solutions.

Oils of Aniseed.
„ Bay.
„ Bergamot.
„ Bitter Almonds.
„ Cassia.
„ Cloves.
Carvol, oxidised derivative of carraway oil.
Myristicol, the same from nutmeg oil.
Patchouli, blue oil of.
Oils of Peppermint.
„ Pimento.
„ Thyme.

For the most part these latter substances are known to contain the aromatic nucleus as an essential part of their constitution. Thus the oils of bay, pimento, and cloves, contain the substance eugenol, or $C_6H_3.OH.OCH_3.C_3H_5$; oil of cassia consists of cinnamic aldehyde, $C_6H_5.C_2H_2.COH$; oil of aniseed of anethol, $C_6H_4.OCH_3.C_3H_5$; and oil of thyme contains thymol, $C_6H_3.OH.CH_3.C_3H_7$, as well as much cymene, $C_6H_4.CH_3.C_3H_7$.

Some other oils, such as bergamot and oil of peppermint, as likewise the bodies menthol, carvol, and myristicol, have an unknown constitution. The three latter substances are known to be isomeric ("Journal of the Chemical Society," vol. xxv, p. 1).

Great interest is attached to our examination of these bodies, since we consider it to be proved from the character of the spectra they transmit that the nucleus of menthol is a terpene, while the benzene ring is the inner basis of carvol and myristicol. Bergamot appears to be a terpene mixed with some derivative of the aromatic series, but the oil of peppermint on the other hand is essentially a substance belonging to this latter class. The refraction equivalents of carvol and myristicol are abnormal, like those of benzene derivatives, a fact which confirms our conclusions regarding the constitution of these substances.

The following is a summary of our observations with regard to the terpenes:—

1. The terpenes with the composition $C_{10}H_{16}$ possess, in a high

degree, the power of absorbing the ultra-violet rays of the spectrum, though they are inferior in this respect to benzene and its derivatives, to which class of bodies they are so closely allied.

2. Terpenes with the composition $C_{15}H_{24}$ have a greatly increased absorptive power for the more refrangible rays, that is to say, they withstand dilution to a greater extent the greater the number of carbon atoms in the molecule.

3. Neither the terpenes themselves nor the oxidised or hydrated derivatives occasion absorption bands under any circumstances when pure, but always transmit continuous spectra.

4. Isomeric terpenes transmit spectra which generally differ from one another in length, or show variations on dilution.

5. The process of diluting with alcohol enables the presence of bodies of the aromatic series to be detected in essential oils, and even in some cases the amount of these substances present may be estimated.

Several diagrams in illustration of the kind of absorption exerted by the different substances are presented with the complete paper.

III. "Preliminary Note on Magnetic Circuits in Dynamo- and Magneto-Electric Machines." By Lord ELPHINSTONE and CHARLES W. VINCENT, F.R.S.E., F.C.S., F.I.C. Received July 26, 1879.

The experiments which form the subject of the present note were made in connexion with an investigation as to the best form for the construction of a dynamo-electric machine, intended to furnish currents of high intensity in great quantity. The principle deduced applies equally to magneto-electric machines.

The source of power in all dynamo-electric machines being electro-magnets whose cores are already slightly magnetic, it appeared to us necessary to consider the conditions under which the initial force of such machines is best obtained.

For this purpose we made use of a U electro-magnet having a core of soft iron 2 inches in diameter and 36 inches long. The arms of the U were 4 inches apart. The exciting helices were two sheet copper reels, 12 inches long, fitting closely upon the uprights of the U, but readily removable. Each of these reels was coiled with 200 yards of No. 14 double covered copper wire.

Two cores of soft iron, of the same diameter, and each $12\frac{1}{4}$ inches long, and which could be magnetised by the same helices, were also employed.

The principal armature was of soft iron, 8 inches in length, by 2 inches in width, and 1 inch thickness, rounded at the ends. Its face

fitted approximately close to the poles of the U magnet, whose faces it completely covered when placed upon them.

Other armatures and magnets were employed, the form of which we propose to describe in a future paper.

The iron of which the U and the straight cores were made was found to be exceedingly plastic as regards molecular magnetic polarity. In a few seconds after the cessation of an electric current from twenty quart Bunsen cells acting through the above helices, they were incapable of attracting and holding even fine iron filings.

The U magnet tested with a suspended magnetic needle was found to retain some magnetic polarity after many days; in fact, it is doubtful if the magnetism ever entirely disappeared, except when the core was subjected to special treatment.

On the other hand, the straight cores lost their induced magnetism more rapidly, and when, having been demagnetized either by time or by the mode described further on, they were placed in the line of the magnetic dip, they showed poles in accordance therewith; and on reversing the position of the core, these poles were immediately reversed without its being necessary to resort to striking the bars or other means of putting them in a state of vibration. It was thus demonstrated to our minds that if iron of similar quality, and in this form, were made use of for the electro-magnet cores in a dynamo-electric machine, the initial force producing the electric currents of the machine could not be due to residual magnetism, but rather to the lines of magnetic force of the earth.

The current from four Bunsen cells sent round the U magnet fixed the armature so firmly that it could not be pulled, or even slid off, by the utmost exertion of one man's strength.

On breaking battery contact, if both poles were completely covered, a direct pull failed to separate armature and magnet. The armature could, however, though with difficulty, be slid off; the difficulty of movement greatly increasing as the edge of the poles was approached. For instance, on attempting to slide the armature off the north, the south, or both poles, the resistance became greater as the point of final communication between the poles through the moving armature was approached. This was found to be the case whatever time had passed between the rupture of contact and the first movement of the armature. (Sometimes many days elapsed.) In very many experiments it was found, moreover, that, provided neither pole had been completely uncovered, on sliding back the armature to its normal position, the magnet, which with its stand and coils weighed over 58 lbs., could be lifted by it.

A current from four Bunsen cells, almost momentary in duration, sufficiently magnetised the core to produce all the above effects.

If, whilst the current flowed round the U magnet, the armature

rested on one pole only, it was of course strongly held; but on breaking contact it was at once set free, and fell off if not balanced; the magnetism of the U core immediately falling to its minimum, as shown by suspended test needles. If, however, the most minute point of connexion existed between the armature and the other pole, in addition to its complete contact with the one it covered, it continued to be firmly held long after battery contact was broken.

It being thought that possibly the effects described were partly due to molecular attraction of the iron atoms when brought into close contact under magnetic stress, the poles were coated with a layer of tallow, but if this was sufficiently thin, the magnet could still be lifted by the armature after breaking battery contact. When the tallow was broken into small lumps, allowing light to be seen between magnet and armature, the same result was obtained.

Thus absolute metallic contact was found to be unnecessary for the retention of a considerable amount of magnetism by the U core and its armature, when in magnetic circuit. With a piece of writing paper interposed between the poles and armature, they were held together with great force long after battery contact was broken; but when the distance was increased by the interposition of cards, nails, or wires, to $\frac{1}{16}$ of an inch, the residual attractive force was very much lessened. When the magnetic circuit becomes more open the residual magnetism dies away in about the same proportion as the attractive force of core and armature, whilst under the influence of the battery current it becomes less when the distance between them is increased.

Interposition of thick glazed note paper caused such a diminution of the residual magnetism that the magnet could no longer be lifted by the armature.

The experiment was varied by putting lengths of fine silk thread straight across between the armature and the magnet; in this, as in the former experiments, the armature was firmly held, and the magnet could be lifted by it. There was no point of actual metallic contact, and light could be seen over both magnetic fields, except at the thin lines where the silk threads were. The 58 lb. magnet, when lifted by the armature, was thus literally suspended in the air (like Mahomet's coffin) by the magnetism remaining in the almost closed circuit, and this long after the exciting electric current had ceased. (The experiments were made at intervals of four hours, twenty-four hours, three days, four days; the armature had always ultimately to be wrenched off.)

The same result was obtained with plates or slips of zinc, copper, platinum, silver, and aluminium foils, gutta-percha tissue, embroidery cotton, &c., and appeared to depend entirely on the distance between poles and armature, irrespective of the nature of the interposed body.

When the straight cores were placed on the poles of the U magnet, and a current passed round the latter, attraction ceased the moment the battery contact was broken; but if, while the current was passing, the armature was placed on the poles of the cores, the whole system was firmly held together, though the current no longer flowed.

There would appear to be no limit to the length of time during which the stored-up magnetic force exerts itself in such metallic circuits (closed, or nearly so) as are described above, for it was found that, after periods varying from one to fourteen days from the time of a momentary passage of an electric current round the cores, the attractive force was as great, or even greater, than at the first moment.

A small electro-magnet, U-shaped, with limbs 6 inches long, having a core of $\frac{3}{4}$-inch iron, and helices consisting of 4 layers of No. 16 covered copper wire, had for its armature a similar U core uncoiled. The uncoiled U was hung up, and the electro-magnet held beneath it, the poles of each being opposed: a current from four Bunsen cells was then sent through the coils for a few seconds. Not only did the electro-magnet (weighing, with its coils, several pounds) remain firmly attached to its armature, but the hanging on to it subsequently of 8 pounds additional weight failed to detach it.

A further proof of the large amount of magnetism held captive in a circuit thus closed was afforded by the following experiments. On connecting the ends of the wires from the helices with a galvanometer and resistance-coil, deflections varying from 40° to 90° were obtained with a resistance of 1,700 ohms in circuit each time the armature was forced away from the poles of the large U magnet, after the passage of a current from four cells of a few seconds' duration. By careful manipulation, sparks between the ends of the helix wires were also obtainable each time the closed magnetic circuit was opened. (In one case a week had elapsed betwixt the passage of the current and obtaining of the spark.)

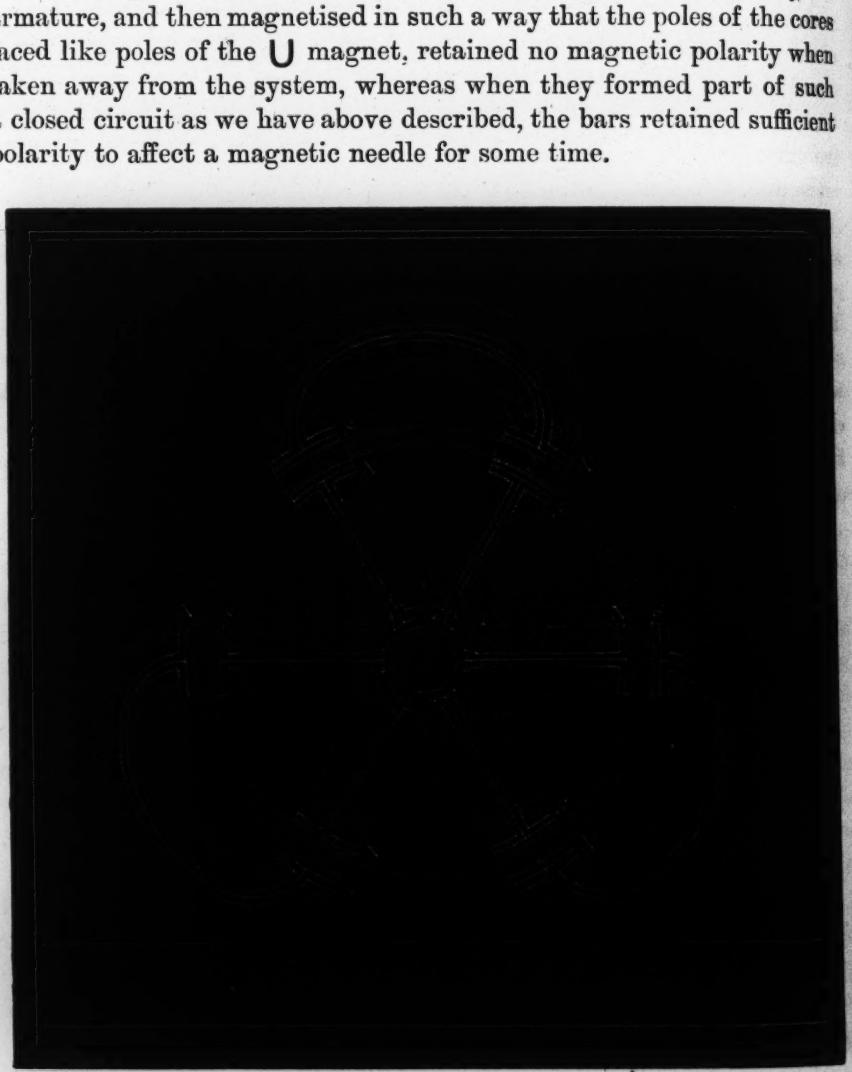
In all these experiments, when the circuit was completely closed there was no external magnetism apparent, but on slightly breaking contact between the poles and armature, magnetic poles could be detected. Slight irregularities of the surfaces in contact likewise caused the development of poles.

A heavy magnetic needle, 4 feet distant from the magnet, if deflected by the U magnet, uncovered by its armature, 45°, would fall back to 5° when the current ceased. If the poles were now covered by the armature, the needle went to 0°. Passage of the current from four cells would now give a deflection of about 38°; on the current ceasing the needle would come back to 0°, and rise again to 5° on removal of the armature: but if, instead of immediately pulling off the armature, the two ends of the wires of the helices were connected

together, and then the armature was forced off, the needle would swing 20° and fall back to 5° very slowly (in about fifteen minutes).

If both poles of the U magnet were caused to be of the same name, and the armature placed upon them, there was no attraction after breaking battery contact.

The straight cores, if placed upon the U magnet connected by the armature, and then magnetised in such a way that the poles of the cores faced like poles of the U magnet, retained no magnetic polarity when taken away from the system, whereas when they formed part of such a closed circuit as we have above described, the bars retained sufficient polarity to affect a magnetic needle for some time.



We may here remark that the attractive force of electro-magnets for each other, in what we call open circuit, is not nearly as great as in a closed circuit. For instance, the U magnet could not be lifted by the straight cores placed upon its poles, even with a current from six Bunsen cells running round the helices; but on bridging the circuit with the armature the whole mass, weighing 82 lbs., could be raised from the ground with the current of only two cells, and quite irre-

spective of the position of the exciting helices, whether both were on the magnet, both on the cores, or one on the magnet and one on a core.

From the foregoing experiments it appears clear that the more near the approach to a closed magnetic circuit, the stronger is the field of force, and the longer is retained the magnetism of the mass or masses of iron constituting the circuit. The same rule holds good with regard to permanent magnets. In closed circuits the attractive force is at its height, and diminishes in intensity as the magnetic field is more extended. But the parallel goes beyond this, for the more open the magnetic field, the more rapidly is the magnetic force itself dissipated.

These principles have guided us in the construction of a dynamo-electric machine of whose magnetic circuits we here present a sketch, and which we hope to describe more fully in a future paper.

In the accompanying diagram six fixed electro-magnets are shown, having alternate poles, opposite to which, and at a very short distance, are placed three other electro-magnets so arranged with opposing poles as to form three nearly closed circuits. Coils of wire are made to revolve so as to cross the intervals between these opposing poles, and the electric currents induced in the moving coils are made to pass round the electro-magnets.

IV. "Further particulars of the Transit of Venus across the Sun, December 9, 1874; observed on the Himalaya Mountains, Mussoorie, at Mary-Villa Station, Lat. 30° 28′ N., Long. 78° 3′ E., height above sea 6,765 feet, with the Royal Society's 5-inch Equatoreal." Note III. By J. B. N. Hennessey, F.R.S. Received October 4, 1879.

1. The object of the present note is to add to Notes I and II* some particulars of the transit not detailed in those notes. The latter contained only sufficient extracts from my observatory notes in connexion chiefly with the three contacts which I observed; as, however, various other facts, besides the contacts, were developed in course of the transit, and elicited remarks from me at the time, it seems desirable that a complete transcript of these observatory notes should also be put on record; both in connexion with what hereafter follows, and also to meet any possible future requirements of details, such as expressed by Captain Tupman in his discussion of the mean solar parallax.†

* See "Proc. Roy. Soc.." Vol. xxiii, pp. 254, 379.

† Royal Astronomical Society, "Monthly Notices." Vol. xxxviii, p. 452.

Observatory Notes.

1874. December.								Remarks made by Observer at the time.
Chronometer Time.				Greenwich Time.				
d.	h.	m.	s.	d.	h.	m.	s.	*Ingress.*
								Beautifully clear morning. No haze. Eye-piece 125. Two coloured glasses giving a neutral or bluish field. Missed the 1st external contact.
8	19	18	0	8	14	6	35	Venus' edge on sun boils somewhat; it is, however, distinctly visible.
		20	0			8	35	Venus' outer edge, *i.e.*, against sky, distinctly visible, because of a narrow edging, or ring of light, around some 30° of that part of it furthest from the sun.
		20	50			9	25	The light ring now extends around the whole of that part of Venus' edge against the sky, and is some 3″ in width; it is brightest where furthest from the sun.
		22	33			11	8	The light ring against sky is well seen, and looks undeniably like Venus' atmosphere; width some 4″; definition excellent.
		23	0			11	35	The light ring against the sky can be plainly traced *in continuation* around Venus on the sun, where however it is made visible chiefly by the *movements* occurring in it of minute bright specks, and also because it boils. The entire ring is full of these minute bright specks, which appear and disappear rapidly, dancing about with little flashes. Definition excellent.
		24	28			13	3	Light ring against sky wider.
		25	10			13	45	,, ,, quite distinct.
		26	26			15	1	,, ,, quite bright.
		28	34			17	9·0	Transit of Venus' dark edge across the sun's limb, *i.e.*, 1st internal contact.
		29	10			17	45	Transit, by estimate, of outer edge of Venus' light ring (that was against sky) across the sun's limb. No ligament or pear-drop seen, *though expected and carefully watched for.*
	22	0	0	8	16	48	36	Brilliantly clear sky; light breeze from south.
		32	0		17	20	36	Spectroscope set up, adjusted, and used. Slit placed across Venus' *centre* gave a black band all along the length of solar spectrum. Slit placed tangential to Venus' disk, gave a faint, narrow glimmer of light, slightly brighter than solar spectrum, instead of black band; no displacement of solar lines in glimmer.
								Egress.
								Eye-piece as before, 125. Of the two coloured glasses used for Ingress, one

1874. December.								Remarks made by Observer at the Time.
Chronometer Time.				Greenwich Time.				
d.	h.	m.	s.	d.	h.	m.	s.	
								Egress. now changed for a red glass; field now red and less lighted than at Ingress. Definition sharp and excellent. No boiling visible around sun's limb; some boiling round Venus' lower limb and still less around upper limb. Removed the red glass and substituted a blue one, when the boiling round Venus became quite visible. Removed the temporary substitute and restored red glass, as before, and thus watched the further progress of the transit.
8	23	9	0	8	17	57	36	About $\frac{1}{4}$ of Venus' diameter from contact. No ligament visible.
		11	38		18	0	14	About $\frac{1}{8}$ of Venus' diameter from contact. No ligament visible.
		12	40			1	16	About $\frac{1}{8}$ of Venus' diameter from contact. No ligament visible.
		15	8			3	44	Less than $\frac{1}{12}$ of Venus' diameter from contact. No ligament visible.
		16	0			4	36	About $\frac{1}{20}$ of Venus' diameter from contact. No ligament visible.
		16	57			5	32·6	2nd internal contact.
		22	0			10	36	Light ring now visible around Venus' limb against sky.
		34	0			22	36	No boiling around Venus' or sun's limb.
		37	30			26	6	Venus' limb against sky only faintly seen.
		40	0			28	36	Light ring has been invisible for some time; was seen for only a short time after 2nd internal contact.
		44	14			32	49·6	2nd external contact. No ligament visible thereafter or at any time before.

2. This record was obtained by means of the following agency. My friend, Mr. W. H. Cole, M.A., audibly counted seconds, and named the minutes as completed, from a large chronometer, before which he was comfortably seated, say six feet from the equatoreal. Baboo Cally Mohun Ghose, with paper and pencil, took up a position by my side; he mentally followed Mr. Cole's counting, and noted down the instant I made a remark, together with the words I uttered. In the absence of remarks, the Baboo noted the number of each complete minute as it was declared, a reckoning which was checked by inspection of the chronometer and otherwise from time to time, so as to render the adoption of a *wrong* minute practically impossible. Thus all that remained for me to do was to look intently through the telescope of the equatoreal and declare exactly what I beheld. We three individuals were all enclosed within the canvas walling of an observatory tent

only eight feet square, the top of the tent being removed, No sound broke the enforced stillness that prevailed; the sky was brilliantly clear, and all cause for anxiety, as to clouding over, or the driving of the equatoreal, or on any other account, was completely absent; while the events in question were plainly seen to progress so gradually and deliberately that there was not the smallest occasion for hurry or confusion of any kind, nor did any occur. Unlike a solar eclipse, when every second is of the utmost importance, there was plenty of time for every purpose. Under these circumstances, I repeat, it was practically impossible that any blunder in recording could be committed.

3. Subsequently, when Colonel J. F. Tennant, F.R.S., was reducing his observations, taken at Roorkee, he wrote to me,* pointing out that there was apparently a blunder of three minutes in the recorded time of my first internal contact. I was well aware of this anomaly, and had previously alluded to it in Burlington House, when taking part in the discussion that followed the reading of my Notes I and II before the Royal Society in 1875; so that with the full knowledge I possessed of all the facts of observations, I found it out of my power to adopt Colonel Tennant's very natural suggestion, that the three minutes in question were due to a blunder in recording.

4. It appears desirable to add a few words as to this anomaly. If due to a record-blunder, then, it will be seen from the observatory notes preceding, the identical three minutes' blunder must have been repeated in no less than eight independent preceding times recorded; and this notwithstanding that, from our contiguity of position, both Baboo Cally Mohun Ghose and myself availed ourselves of ample opportunity to check Mr. Cole's declarations of complete minutes; a conclusion which amounts to assuming practically that three individuals independently concurred in the erroneous records of nine different times, which were equally wrong by three minutes. This is by no means probable.

5. If, on the other hand, the anomaly be ascribed to blunder on my part in visible recognition of the contact, or briefly to contact-blunder, the following evidence in my favour may be adduced by comparing my intervals of contact with those observed by my friend the (late) Rev. H. D. James, M.A., who was stationed only nineteen miles from me to the north-west.†

* See also his Report to Government, p. 41. 1877.

† At Chakatra, lat. N. 30° 43′, long. E. 77° 54′, height 7,300 feet above sea "Proc. Roy. Soc." Vol. xxiii, pp. 381–384.)

Contact.	Hennessey.						James.					
	Time.			Interval from 1st.			Time.			Interval from 1st.		
	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.
1st Internal ..	14	17	9	..			7	41	20			
2nd „ ..	18	5	33	3	48	24	11	30	15	3	48	36*
2nd External	18	32	50	4	15	41	11	57	25	4	15	44*

Mr. James was not in a position to obtain absolute time with any great accuracy, but this does not vitiate his *intervals*. There is every reason to accept these as accurate, for Mr. James was quite familiar with the telescope, which was his own property, and was accustomed to study celestial objects. The accordance of these intervals (by 12 seconds and 3 seconds) is sufficient proof, apart from needless reduction, that if a contact-blunder of three minutes was committed in my first time, about the same occurred in Mr. James's. This, as a blunder, is, to say the least, highly improbable.

6. I think, therefore, it may be claimed for both Mr. James and myself that neither of us committed any blunder, as such, whether of record or contact. On the other hand, I confess my inability, at present, to account for the anomaly.

7. Turning briefly to the preceding observatory notes, it will be seen that, for purposes of emphasizing the visible absence of the expected ligament, I roughly estimated the intervals from 2nd internal contact (when the ligament was intently looked for) on five occasions in terms of Venus' diameter; thus, $\frac{1}{4}$th to $\frac{1}{20}$th diameter. If, for purposes of very rough comparison, we contrast these roughly estimated fractional diameter-intervals, with corresponding fractions of the observed interval between 2nd internal and 2nd external contacts, there result as errors in the former:

Error of	$\frac{1}{4}$th	diameter	+68	seconds.
„	$\frac{1}{6}$th	„	+46	„
„	$\frac{1}{8}$th	„	+52	„
„	$\frac{1}{12}$th	„	−27	„
„	$\frac{1}{20}$th	„	−25	„

which is confirmatory to some extent of the absence of gross error in the times of these two contacts.

Mr. James's letters to me in original is attached.

Excerpt from Mr. H. D. James's letter, above referred to.

"Chakatra, N.W.P., December 9, 1874.

"Through your kindness in sending me a diagram of the planet's

* Intervals corrected for watch rate, giving "perhaps a minute in 12 hours."

progress across the sun's disk I had as satisfactory a view of it as was possible. My son was with me. At 6.56 we noticed the sunlight on the snowy range. Ten minutes and twelve seconds after that we saw the rim of the sun rising above the mountain which intervened between us and the horizon. We kept an eye on Mussoorie and Landour, and thought they had but two minutes' advance of us in seeing the sun. Eighteen minutes and ten seconds after our first glimpse of the sunlight, Venus began nibbling at the rim of the sun. Between this and her entire entrance on to the disk was twenty-seven minutes and ten seconds; that is, we saw her external contact at 7° 14′ 10″, and her internal contact at 7° 41′ 20″. When she was about half way on, we both noticed a fringe of white light illuminating that rim of the planet which was yet on the dark sky. When she went off, we noticed the same fringe of light, but for a much shorter time, and when only about one-eighth of her had passed the sun's disk. The internal contact for departure took place at 11° 30′ 15″, and the external at 11° 57′ 25″, as near as it was possible to say, for there was a sort of flickering, which rendered it difficult to fix on the precise moment of contact.

"The spots on the sun were but insignificant, in magnitude about the same as yesterday, though in position very different."

V. "Preliminary Report to the Committee on Solar Physics on the evidence in favour of the Existence of certain Short Periods common to Solar and Terrestrial Phenomena." By BALFOUR STEWART, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and WILLIAM DODGSON. Communicated to the Royal Society at the request of the Committee. Received October 23, 1879.

1. In a previous report to this Committee ("Proc. Roy. Soc.," vol. xxix, page 106) we exhibited a method of detecting the unknown inequalities of a series of observations, and we gave some evidence that the temperature-range and the declination-range at Kew Observatory are both subject to the same short period inequalities, the particular periods investigated being those around 24 days.

To confirm this evidence we have extended our researches, although in a subject of such magnitude they must still be regarded as of a preliminary nature. In our present paper we investigate after the method described in our first report the following ranges:—

I. Diurnal Ranges of Temperature of Air at—

α Kew (already given).
β Toronto.
γ Utrecht.

II. Diurnal Ranges of Magnetic Declination including Disturbances at—

α Kew.
β Prague.

III. Sun-Spot Inequalities of the period of 24 days as far as this may be obtained from existing records.

For the Toronto ranges we are indebted to the kindness of the Council of the Meteorological Office, and of Mr. Kingston, Director of the Toronto Observatory. We have extracted the Utrecht and Prague ranges from the published records of these observatories, while for the Kew declination-ranges, including disturbances, we have to thank the Committee of the Kew Observatory.

2. To begin with the Toronto and Utrecht temperature ranges, these have been treated *exactly* in the way in which we treated the Kew temperature-range, so that it will only be necessary to exhibit the result of our treatment in tables which correspond with Table VI of our previous report.

Table I.—Equalised Yearly Inequalities of Toronto Temperature-range corresponding to period 24·00 days.

Year	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	−36	−51	−62	−70	−66	−40	+ 1	+55	+85	+85	+62	+20	−11	−25	−34	−27	−28	−22	− 5	+24	+56	+53	+39	− 3
1859	−22	−24	−14	+ 7	+21	+20	+ 0	−30	−47	−48	−42	−18	+ 2	+17	+49	+69	+87	+79	+34	− 3	−40	−44	−27	−26
1860	−18	− 1	+15	+ 6	+ 5	−11	−21	− 2	+ 3	+17	+41	+52	+77	+93	+91	+68	+29	−15	−61	−90	−97	−82	−65	−34
1861	−13	−15	−20	−30	−16	− 2	+16	+33	+18	+ 1	−17	−26	−14	− 7	+ 8	+ 9	+ 2	+12	+ 9	+11	+23	+11	+ 4	+ 3
1862	+27	+65	+90	+86	+65	+27	−20	−33	−18	+ 5	+45	+62	+57	+33	−12	−50	−74	−79	−71	−65	−56	−52	−32	− 0
1863	+41	+22	+ 1	− 3	− 4	− 1	+10	+ 8	− 2	−18	−53	−81	−98	−109	−93	−71	−29	+19	+48	+83	+93	+86	+87	+64
1864	−15	− 4	+14	+40	+59	+68	+59	+35	+ 4	−17	−24	−20	−17	−20	−24	−23	− 7	+ 9	+ 8	− 4	−24	−36	−33	−28
1865	−83	−83	−68	−35	+ 3	+34	+66	+84	+88	+77	+45	+ 8	−22	−45	−43	−35	−12	+18	+31	+45	+27	− 2	−31	−67
1866	+15	−16	−36	−59	−55	−36	−15	+16	+28	+31	+27	+ 4	−14	−26	−26	−14	+11	+17	+ 5	+ 9	+14	+30	+45	+45
1867	−26	−16	−11	+ 9	+28	+25	+23	+22	+30	+43	+45	+41	+25	+ 8	+ 9	+ 5	− 5	−17	−36	−51	−51	−43	− 31	−26
1868	+29	+ 9	−11	− 7	+ 6	−12	−15	−39	−79	−91	−95	−85	−47	−12	−17	−10	− 5	+ 4	+56	+87	+96	+102	+82	+54
1869	−57	−57	−48	−30	+ 9	+44	+60	+76	+77	+57	+43	+15	−14	−25	−27	−12	+ 7	+16	+14	− 7	−22	−35	−43	−41
1870	−48	−53	−52	−47	−33	−26	−35	−29	−31	−39	−24	−17	+ 5	+42	+60	+79	+83	+76	+67	+45	+24	+ 1	−17	−31
1871	+31	+ 6	+ 2	−18	−14	− 7	−12	−21	−29	−33	−43	−50	−54	−46	−16	+ 9	+28	+34	+33	+34	+39	+46	+35	+46
1872	−68	−57	−48	−24	+16	+57	+95	+95	+66	+12	−33	−50	−40	− 5	+19	+30	+24	+18	+19	+13	+ 2	−26	−52	−63
1873	+66	+39	+13	−22	−25	−16	−16	− 1	− 9	−30	−32	−53	−67	−75	−80	−57	−30	+ 7	+35	+41	+66	+75	+79	+92
	−177	−236	−235	−197	−1	+124	+196	+269	+184	+52	−55	−198	−232	−202	−136	−30	+81	+176	+186	+172	+150	+84	+40	−15
																							(sum = 3428)	

Table II.—Equalised Yearly Inequalities of Utrecht Temperature-range corresponding to period 24·00 days.

Year	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	−77	−54	−28	− 7	+13	+24	+52	+86	+104	+105	+65	+36	+24	+23	+46	+49	+30	+ 2	−43	−72	−94	−100	−92	−92
1859	−29	−20	−10	−13	− 8	− 4	− 7	− 7	−11	−12	− 3	+22	+30	+26	+ 2	−29	−33	− 9	+29	+49	+50	+19	−10	−22
1860	−21	−19	−15	−34	−55	−74	−79	−54	−18	+23	+40	+41	+21	− 6	− 3	+12	+45	+77	+74	+56	+29	− 2	−17	−21
1861	+24	+18	+18	− 2	− 5	− 2	−11	−15	−37	−46	−42	−37	−22	− 6	+ 3	+ 9	+22	+15	+20	+20	+18	+22	+11	+25
1862	+28	− 8	−39	−65	−85	−84	−78	−76	−69	−52	−29	+18	+58	+74	+75	+64	+52	+46	+39	+20	+17	+24	+32	+38
1863	+ 0	−21	−30	−23	+ 3	+28	+46	+51	+40	+49	+62	+68	+67	+36	−15	−47	−78	−91	−69	−60	−32	− 1	+ 5	+12
1864	+41	+ 1	−48	−91	−107	−112	−96	−70	−61	−34	− 1	+39	+67	+75	+63	+25	+13	+ 5	+ 1	+28	+46	+65	+81	+70
1865	−35	−43	−37	−18	+10	+20	+10	+ 3	− 1	− 4	− 2	−20	−32	−35	−22	+ 3	+ 6	+32	+33	+37	+52	+30	+18	− 5
1866	+66	+65	+51	+39	− 5	−37	−55	−58	−36	−14	+ 3	+12	+12	+ 3	−16	−38	−57	−51	−28	− 0	+19	+35	+45	+45
1867	−35	− 4	+44	+92	+138	+149	+120	+86	+43	+14	− 1	−24	−27	−55	−67	−60	−66	−48	−50	−51	−53	−55	−47	−43
1868	−24	+36	+73	+89	+80	+40	+17	+ 2	+ 0	+20	+36	+47	+50	+30	+ 3	−18	−35	−34	−38	−51	−71	−92	−91	−69
1869	− 4	+ 6	+ 7	+13	+18	+15	+13	+ 1	− 1	+ 9	− 7	−26	−39	−55	−21	+21	+43	+57	+26	− 3	−17	−33	−18	− 5
1870	−17	+23	+39	+43	+ 9	−27	−51	−68	−55	−36	− 5	+25	+45	+53	+53	+40	+34	+38	+26	+16	−15	−51	−63	−56
1871	+76	+71	+40	+15	−18	−51	−58	−62	−49	−44	−45	−40	−49	−52	−42	−33	−14	+11	+23	+33	+54	+65	+82	+87
1872	+ 3	+ 7	+10	+ 8	+ 2	− 7	−24	−25	−25	−20	− 9	−17	−21	−22	−10	+25	+44	+52	+43	+12	− 3	−10	−10	− 3
1873	+32	+ 2	−20	−54	− 86	−86	−83	−69	−41	−33	−19	−15	−17	+ 8	+32	+71	+94	+89	+74	+36	+18	+20	+17	+30
	+28	+60	+55	− 8	−96	−208	−284	−275	−217	−75	+43	+129	+167	+97	+81	+94	+100	+191	+160	+70	+18	−64	−57	− 9
																							(sum=2586)	

3. Applying the method already described by us to the results in Tables I and II, and borrowing for comparison the results of Table VII of our previous communication, we obtain the following table:

Table III.—Exhibiting the Results of our method applied to Table VI of our previous and Tables I and II of our present communication.

Divisions from normal.	Exact period in days.	Magnitude of inequality. Kew.	Utrecht.	Toronto.
−7·0	23·5400	2100	1922	2934
−6·5	23·5729	3093	3080	1422
−6·0	23·6057	4700	3950	2252
−5·5	23·6386	4025	3980	4446
−5·0	23·6715	1386	3030	3939
−4·5	23·7043	1887	2624	5246
−4·0	23·7372	3910	2166	3638
−3·5	23·7700	3915	1780	2622
−3·0	23·8029	3140	3992	2148
−2·5	23·8357	2771	4540	3337
−2·0	23·8686	4234	4578	3422
−1·5	23·9014	5921	4624	2906
−1·0	23·9343	5518	3878	3098
−0·5	23·9671	2374	2572	2772
0·0	24·0000	3912	2586	3428
+0·5	24·0329	5135	3958	2863
+1·0	24·0657	4516	2984	1678
+1·5	24·0986	2157	3394	3902
+2·0	24·1314	2378	5392	3216
+2·5	24·1643	3795	5690	3360
+3·0	24·1971	3926	3350	4274
+3·5	24·2300	3043	2520	2728
+4·0	24·2628	2520	4342	2377
+4·5	24·2957	3004	5802	3258
+5·0	24·3285	4302	5572	3601
+5·5	24·3614	4761	5146	2400
+6·0	24·3943	5824	3832	2906
		98247	101284	84173

4. It will be noticed that there is a very striking likeness between the three vertical columns in Table III. This will be seen more distinctly by reference to Plate 1, in which figures 2, 3, and 4 represent the numbers of these three columns; fig. 2 denoting that

for Kew, fig. (3) that for Utrecht, and fig. 4 that for Toronto. This means that the temperature-ranges at these three places when treated in a precisely similar manner indicate the existence in each of inequalities of the same periods. The results are not, however, precisely the same for each, which may perhaps be accounted for by certain local influences which have not been entirely obliterated, but which might be overcome by a longer series of observations.

The sums of the three vertical columns too are not very different from each other. We have, Kew=98247, Utrecht=101284, and Toronto=84173, a result which might seem to indicate that the magnitude of these inequalities is nearly the same at the three stations. All this is in favour of their cosmical origin.

5. The next point is to obtain the average results for the three observatories. To obtain this it will not answer to add the three columns together horizontally, and then divide by 3. For although these periods may have a cosmical origin, and be due to the sun, yet the corresponding phases may not be simultaneous at those places. Indeed, this is rendered probable by the fact observed by Toynbee and others that certain weather changes which are in a sense caused by the sun appear to travel from west to east. Now, if we should find that these inequalities take the same course it is not impossible to imagine that they may be allied to, or even in a sense identical with those other well-known changes of weather. We may be as it were on the track of those changes which may perhaps be due to solar inequalities.

6. How then are we to decide whether the various phases of these inequalities occur at Toronto before they occur at Kew? or occur at Kew before they take place at Utrecht? in fine, how are we to ascertain that these appearances travel from west to east?

This may be ascertained in the following manner. Any one of the numbers in Table III is the sum of the 24 terms of an inequality which have been added together without respect of sign. Thus the number for 0·0 is for Kew, 3912; for Utrecht, 2586; for Toronto, 3428. It does not, however, follow that if we add algebraically together the three inequalities corresponding to these three numbers, we shal obtain an inequality the sum of whose terms shall be equal to the sum of these three numbers. For the three inequalities, even if precisely the same in type, may not have their corresponding phases occurring together. The signs of the numbers we add together may therefore sometimes be different, and we shall then have to subtract the one from the other. Thus again if there be a want of simultaneity of phase in two such inequalities, the algebraic addition together of the two will give a result less than the sum (without reference to signs) of the 48 terms, and this falling off will be greater the greater the want of correspondence in phase. Now let us regard Kew as

our base station. We have in all 27 inequalities between $-7{\cdot}0$ and $+6{\cdot}0$, the sums of which represent the numbers of the vertical column headed Kew in Table III. In like manner for Toronto we have 27 similar inequalities. Let us now add together algebraically the inequalities of Kew and the corresponding inequalities of Toronto, under the supposition that the phases are simultaneous at the two places. We thus get a series of 27 inequalities representing the united result of the two observatories. Treating these in the same way in which single observatories are treated in Table III, we get 27 sums, and now finally let us add these sums together. We get in this particular instance, Kew+Toronto=133657. Let us next, on the supposition that a phase at Toronto occurs 6 days earlier than the corresponding one at Kew, rectify this by pushing each Toronto inequality 6 divisions to the right before adding it to the corresponding Kew inequality. Obtaining as before 27 sums and adding them together, we get as a result 141794.

This then is greater than the result obtained by adding the inequalities together as they stand, and would seem to indicate that by means of thus pushing Toronto to the right we are bringing the phases more nearly into accordance with one another.

7. In the following table we have exhibited the results obtained by this method of treatment.

Table IV.—Algebraic sum of Kew and Toronto Temperature Inequalities.

Kew and Toronto (together)	.....			=133657
,,	(Toronto pushed	6	divisions to right)	=141794
,,	,,	7	,,	=143880
,,	,,	8	,,	=144746
,,	,,	9	,,	=142523
,,	,,	10	,,	=137556

It thus appears that we get the greatest sum, and consequently the nearest approach to similarity of phase when we push Toronto forward, or to the right, 8 divisions—in other words similar phases take place at Toronto about 8 days before they take place at Kew.

Viewing Kew as our standard, we have in like manner compared Utrecht and Kew together. And just as we might expect Toronto to be before Kew, so we might expect Utrecht to be behind it. It will be seen by the following table that this surmise is correct.

Table V.—Algebraic sum of Kew and Utrecht Temperature Inequalities.

Kew and Utrecht	(Utrecht 1 division to right) ..	=165950
,,	(together)	=172807
,,	(Utrecht 1 division to left) ..	=174776
,,	(Utrecht 2 divisions to left) ..	=173997
,,	(Utrecht 3 divisions to left) ..	=164915

It thus appears that we get the greatest sum, and consequently the nearest approach to similarity of phase when we push Utrecht backwards or to the left a little more than 1 division, in other words similar phases take place in Utrecht about a day and a-half after they take place in Kew.

8. In adding together the results of the three observatories, we shall therefore push Toronto 8 divisions to the right, and Utrecht 1 division to the left.

By doing so we obtain the following table :—

Table VI.—In which Toronto, Kew, and Utrecht are added together as now described and the sums divided by 3.

Divisions from normal.	Magnitude of inequality.	Divisions from normal.	Magnitude of inequality.
−7·0	1533	0·0	2709
−6·5	2199	+0·5	3001
−6·0	3208	+1·0	2549
−5·5	3642	+1·5	1734
−5·0	1995	+2·0	1521
−4·5	2362	+2·5	2090
−4·0	2616	+3·0	2412
−3·5	2012	+3·5	2331
−3·0	1439	+4·0	2245
−2·5	2344	+4·5	3476
−2·0	3571	+5·0	3788
−1·5	3808	+5·5	3620
−1·0	3411	+6·0	3280
−0·5	1558		

9. The results of this table are exhibited in Plate 1, fig. 1. It will be seen that the Kew curve (Plate 1, fig. 2) is remarkably like the above mean curve; the Utrecht curve (fig. 3) is not quite so like the mean as the Kew curve, while the Toronto curve (fig. 4) is the

least like. But all these curves have a very considerable likeness both to the mean curve and to one another. In the following table we have compared the six common prominences A, B, C, D, E, F, giving the numerical and time value of each for each observatory, as well as for the mean curve.

Table VII.—Giving the Numerical and Time Values of the various Prominences of the Temperature Curves.

	Mean.		Kew.		Utrecht.		Toronto.	
Designation of prominence.	Numerical value.	Time value in days.	Numerical value.	Time value in days.	Numerical value.	Time value in days.	Numerical value.	Time value in days.
A	3650	23·633	4740	23·614	4100	23·624	4450	23·640
B	2640	23·730	4010	23·752	2580	23·710	5250	23·705
C	3810	23·900	5980	23·914	4630	23·895	3560	23·853
D	3000	24·029	5140	24·035	3960	24·033	3530	24·015
E	2430	24·210	4040	24·185	5810	24·154	4280	24·195
F	3790	24·332	4400	24·334	5800	24·298	3660	24·322

10. We have hitherto been comparing together the periods of the chief temperature inequalities as indicated by the various observatories. Let us now compare together these inequalities themselves. For this purpose prominence A may be conveniently represented by taking a mean of the two inequalities for $-6{\cdot}0$ and $-5{\cdot}5$.

Prominence	B	in like manner by a	mean of	$-4{\cdot}5$ and $-4{\cdot}0$
,,	C	,,	,,	$-2{\cdot}0$ and $-1{\cdot}5$
,,	D	,,	,,	$0{\cdot}0$ and $+0{\cdot}5$
,,	E	,,	,,	$+3{\cdot}0$ and $+3{\cdot}5$
,,	F	,,	,,	$+5{\cdot}0$ and $+5{\cdot}5$

Let us perform these operations separately for each of the three stations, and also for the mean of the three as above determined, after having brought them all to the same phase by the method above indicated, and we thus obtain the following table :—

Table VIII.—In which are compared together the various Temperature Inequalities representing A, B, C, D, E, F, at the various Observatories. (K=Kew, T=Toronto, U=Utrecht.)

$A = \frac{(-6{\cdot}0)+(-5{\cdot}5)}{2}$	K+U+T mean=	−136	−174	−163	−155	−102	− 51	− 11	+ 39	+ 54	+112	+158	+195	+238	+197	+147	+ 85	+ 3	− 45	− 83	− 89	− 56	− 37	− 47	− 72
	K =	−207	−219	−175	−124	− 57	+ 11	+ 29	+ 59	+ 75	+ 97	+143	+227	+286	+283	+277	+189	+ 88	− 6	−127	−181	−184	−163	−148	−165
	U =	−128	−125	−108	−131	− 98	− 49	− 10	+ 30	+ 46	+109	+177	+233	+268	+223	+156	+ 96	+ 11	− 44	− 87	−113	−100	−112	−132	−114
	T =	− 71	−178	−209	−211	−152	−116	− 52	+ 26	+ 42	+131	+153	+125	+159	+ 85	+ 6	− 30	− 89	− 84	− 37	+ 26	+116	+164	+139	+ 62
$B = \frac{(-4{\cdot}5)+(-4{\cdot}0)}{2}$	K+U+T mean=	− 94	− 44	+ 23	+ 47	+ 83	+134	+175	+197	+181	+139	+ 90	+ 49	+ 9	− 30	− 80	−100	−112	− 88	− 59	− 72	− 65	−110	−148	−121
	K =	− 62	− 47	− 26	− 40	− 42	+ 9	+ 81	+133	+158	+175	+183	+164	+107	+ 36	− 42	− 74	− 63	− 52	− 49	− 71	− 92	−125	−142	−108
	U =	−110	− 28	+ 55	+ 76	+ 78	+ 56	+ 27	+ 47	+ 78	+123	+154	+150	+120	+ 53	− 44	− 90	−126	− 85	− 22	− 34	− 41	−119	−173	−146
	T =	−111	− 55	+ 40	+106	+215	+336	+417	+411	+307	+117	− 67	−165	−200	−179	−152	−136	−147	−126	−104	−111	− 61	− 85	−129	−110
$C = \frac{(-2{\cdot}0)+(-1{\cdot}5)}{2}$	K+U+T mean=	+ 69	+ 66	+ 37	+ 35	− 0	− 61	− 93	−147	−185	−213	−245	−210	−151	− 76	+ 0	+ 22	+ 33	+ 70	+115	+189	+233	+215	+187	+115
	K =	+ 65	+ 21	− 10	− 51	− 95	−151	−234	−242	−247	−250	−233	−207	−150	− 69	+ 12	+ 56	+ 78	+123	+186	+272	+348	+332	+278	+177
	U =	+ 55	− 5	− 94	− 87	− 68	− 51	+ 31	+ 14	− 41	−131	−244	−245	−195	−102	− 27	− 27	− 43	− 25	+ 39	+190	+290	+317	+293	+156
	T =	+ 88	+184	+215	+243	+162	+ 20	− 77	−213	−267	−257	−257	−178	−106	− 57	+ 15	+ 36	+ 63	+111	+120	+104	+ 61	− 3	− 11	+ 11
$D = \frac{(0{\cdot}0)+(+0{\cdot}5)}{2}$	K+U+T mean=	− 66	− 53	− 68	− 97	−125	−129	− 78	− 13	+ 30	+ 64	+ 68	+ 54	+ 57	+ 72	+103	+144	+167	+144	+ 87	+ 6	− 56	− 98	−112	− 95
	K =	−156	− 85	− 70	− 79	−126	−182	−160	− 91	− 6	+ 78	+145	+180	+180	+196	+216	+222	+248	+215	+120	+ 7	−125	−223	−248	−239
	U =	−132	−138	−136	−159	−183	−161	− 73	+ 22	+131	+146	+158	+110	+ 49	+ 18	+ 45	+ 91	+154	+168	+119	+ 56	− 14	− 78	−115	−123
	T =	+ 91	+ 65	+ 2	− 51	− 65	− 44	− 1	+ 29	− 35	− 75	−101	−128	− 58	+ 2	+ 46	+119	+ 98	+ 50	+ 22	− 44	− 27	+ 6	+ 27	+ 78
$E = \frac{(+3{\cdot}0)+(+3{\cdot}5)}{2}$	K+U+T mean=	+ 28	+ 49	+ 47	+ 36	+ 34	+ 54	+ 47	+ 14	− 41	−121	−158	−121	− 56	+ 10	+ 53	+ 46	+ 37	+ 39	+ 32	+ 18	− 1	− 28	− 19	+ 7
	K =	+135	+133	+110	+ 73	+106	+170	+160	+ 95	− 33	−181	−226	−174	−105	− 54	− 52	− 78	− 76	− 40	− 12	− 17	− 31	− 26	+ 25	+104
	U =	+125	+135	+ 59	+ 21	+ 10	+ 19	+ 36	− 1	− 90	−164	−185	−126	− 53	− 13	+ 8	− 8	+ 11	+ 40	+ 34	+ 19	− 4	− 17	+ 44	+ 99
	T =	−178	−122	− 28	+ 15	− 15	− 26	− 56	− 52	− 0	− 17	− 64	− 62	− 10	+ 96	+203	+225	+175	+116	+ 73	+ 52	+ 33	− 40	−126	−183
$F = \frac{(+5{\cdot}0)+(+5{\cdot}5)}{2}$	K+U+T mean=	+172	+205	+234	+248	+230	+169	+ 62	− 28	−111	−156	−137	−113	− 80	− 78	−110	−150	−175	−170	−152	−100	− 34	+ 30	+103	+146
	K =	+213	+204	+204	+188	+165	+ 95	− 30	−130	−272	−349	−303	−247	−145	− 72	− 67	− 74	− 60	− 40	− 11	+ 45	+ 99	+164	+212	+222
	U =	+199	+277	+333	+379	+396	+332	+188	+ 45	− 66	−108	− 92	− 89	− 93	−151	−204	−264	−313	−293	−269	−214	−122	− 59	+ 47	+142
	T =	+103	+133	+166	+176	+129	+ 81	+ 27	+ 0	+ 5	− 11	− 16	− 4	− 1	− 11	− 58	−111	−152	−175	−174	−132	− 78	− 13	+ 51	+ 74

11. The results of Table VIII are exhibited graphically in Plates 2 and 3.

It will be seen that there is a striking likeness between the corresponding inequalities of the various observatories. Of these Kew is perhaps most like the mean, while Utrecht follows next in similarity; Toronto is least like, but even here the similarity is very apparent.

12. Before leaving this division of our subject we have one remark to make. We do not suppose that certain solar inequalities only affect directly some particular region of the earth from which they are propagated, say from west to east. It seems to us more probable that any solar inequality affects directly all the various stations. But at the same time there may be a progress from west to east of the weather variations caused by these inequalities. Suppose for instance that one of these has the period of 24 days. The temperature-range at Kew will, we may imagine, be directly influenced by the inequality. At the same time it may very probably be influenced by weather changes proceeding from west to east, that have also the same period. These two sources of inequality will be superimposed on each other at Kew, and since each has the period of 24 days, their resultant will have the same period, only in such a case the phase at Kew may be imagined to occur later in time than the corresponding phase of a station to the west, and earlier in time than the corresponding phase of one to the east of Kew.

13. Let us now proceed to discuss our magnetic results. In our last report we applied our method to the Kew declination-ranges, disturbances being excluded. We have since had reason to question the propriety as far as our purpose is concerned, of excluding such disturbances. For these disturbances are known from the researches of Sabine and others to be due to the state of the sun's surface, and Broun in his recent researches has rendered it highly probable that such disturbances indicate short periods of solar inequality as well as the long period of eleven years. In fine, the disturbed as well as the undisturbed ranges are probably influenced by the same solar periods. Now the effect of excluding disturbances will be to cut off the greatest and least values, and thus greatly to diminish the range of inequality. A slight glance at Table IV of our previous communication will render this evident. If we add up the 27 inequalities (from $-7{\cdot}0$ to $+6{\cdot}0$) of this table in which disturbances are excluded, we obtain only 46,040 as against 98,247, which last is the corresponding sum deduced from the Kew temperature ranges.

We have, therefore, been induced to apply our method to the Kew declination-ranges, including disturbances. Subjecting these to the treatment already described, and using always the same sixteen years, we obtain the following table:—

Table IX.—Equalised Yearly Inequalities of Kew Declination-range (including disturbances) corresponding to period 24·00 days.

Year	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	+51	+64	+59	+25	− 2	−37	−50	−37	−20	+ 5	+15	+20	+14	+ 5	+ 5	− 7	−27	−46	−54	−45	−16	+10	+25	+43
1859	−86	−76	−66	−62	−52	−38	0	+57	+98	+120	+105	+55	+23	+ 1	− 2	+10	+11	+20	+26	+23	+ 6	−27	−62	−84
1860	+41	+43	+29	+16	+ 5	−10	−21	−34	−46	−45	−34	−20	+ 4	+19	+15	+14	0	− 3	+ 2	− 6	− 4	− 4	+ 9	+30
1861	−34	−40	−50	−68	−81	−77	−54	−17	+25	+44	+45	+40	+23	+23	+38	+44	+50	+58	+43	+33	+17	− 9	−21	−32
1862	+ 1	−29	−56	−96	−130	−90	−38	+39	+117	+116	+89	+37	−24	−39	−30	− 9	+18	+39	+42	+38	+17	+ 3	− 7	− 8
1863	−18	− 7	− 6	− 1	− 2	+12	+35	+44	+61	+49	+26	+10	−16	−26	−22	−16	+ 2	+21	+15	− 2	−27	−49	−49	−34
1864	+44	+56	+61	+58	+59	+44	+40	+29	+16	+17	+ 3	− 4	−17	−31	−32	−35	−38	−51	−61	−71	−65	−35	−11	+24
1865	−29	−15	+ 2	+ 4	+26	+17	−13	−10	−13	+ 4	+53	+64	+73	+73	+33	+19	+13	−12	−23	−47	−71	−67	−51	−30
1866	−53	−51	−48	−31	− 8	+ 3	+11	+16	+20	+33	+65	+90	+95	+85	+35	−18	−48	−61	−42	−19	− 5	−10	−21	−38
1867	−45	−41	−33	−21	−17	−11	− 4	−11	−23	−30	−35	−33	+ 6	+36	+70	+108	+98	+83	+48	+ 3	−19	−37	−45	−47
1868	−76	−55	−28	+13	+20	+17	+18	+ 3	+10	+27	+38	+54	+55	+50	+51	+42	+28	+ 3	−17	−25	−37	−43	−62	−86
1869	+ 7	−28	−61	−73	−78	−80	−90	−104	−102	−93	−74	−45	−23	+ 1	+35	+74	+108	+125	+123	+109	+88	+75	+67	+39
1870	+36	+10	−17	−32	−26	−17	−18	−22	−32	−37	−30	−17	+ 1	+14	+15	+10	+ 7	+13	+ 7	+ 8	+14	+17	+43	+53
1871	−34	−15	+ 8	+24	+46	+53	+47	+33	+ 9	− 2	+ 2	+26	+68	+95	+97	+64	+ 2	−54	−87	−104	−90	−77	−67	−44
1872	+47	+51	+45	+37	+ 9	+ 5	− 4	− 5	+10	+15	+23	+22	− 1	−16	−30	−35	−30	−28	−30	−35	−35	−27	− 4	+16
1873	+ 7	+22	+ 6	−16	−29	−30	+ 1	+30	+47	+60	+51	+47	+54	+46	+28	+ 1	−42	−63	−62	−54	−37	−35	−24	− 8
	−141	−111	−155	−223	−260	−239	−140	+11	+177	+283	+342	+346	+335	+336	+306	+266	+152	+44	−70	−194	−264	−315	−280	−206
																							(sum = 5196)	

14. We have applied the same process to the declination-ranges of the Prague Observatory, selecting the same series of years. From the published records of that institution we have obtained a series of diurnal ranges in which disturbances are included. There are no self-recording instruments at that observatory, but observations at 18 h., 22 h., 2 h., 10 h., are common to the whole series, and our daily ranges are therefore deduced from these four hours. There is reason to believe that the ranges thus obtained are not greatly different from those which would have been obtained from an hourly series of observations.

Applying our method to the Prague series, we have obtained the following table (see next page):—

15. In applying our method to the results in Tables IX, X, we obtain the following table:—

Table XI.—Exhibiting the Results of our method applied to Tables IX, X, of our present communication.

Divisions from normal.	Magnitude of Inequality. Kew.	Prague.
−7·0	1378	1744
−6·5	3948	2590
−6·0	3774	3772
−5·5	3982	3992
−5·0	3380	3204
−4·5	2210	2350
−4·0	1272	1318
−3·5	3944	3344
−3·0	4144	3276
−2·5	3636	3484
−2·0	3024	3564
−1·5	2002	2296
−1·0	3320	3178
−0·5	5260	3446
0·0	5196	4544
+0·5	4216	5178
+1·0	3108	5175
+1·5	3016	5206
+2·0	2910	3632
+2·5	4226	3764
+3·0	6522	4544
+3·5	6934	5660
+4·0	5104	4256
+4·5	2744	2778
+5·0	1836	1290
+5·5	3524	2752
+6·0	2210	3402
	96820	93739

Table X.—Equalised Yearly Inequalities of the Prague Declination-range (including disturbances) corresponding to period 24·00 days.

Year.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	+37	+56	+58	+45	+22	− 8	−29	−29	−24	−20	−18	−25	−21	− 5	+12	+22	+17	− 3	−33	−39	−31	−19	+ 9	+26
1859	−29	−23	−19	−24	−23	− 3	+19	+55	+81	+97	+99	+73	+52	+17	−12	−29	−45	−47	−42	−36	−35	−41	−46	−39
1860	+85	+36	+26	−11	−45	−80	−98	−83	−46	− 1	+37	+52	+53	+47	+24	+10	− 7	− 9	− 2	0	+11	+ 7	+13	+31
1861	− 1	− 4	−20	−36	−54	−55	−40	−37	−24	−20	−17	+ 6	+35	+57	+75	+74	+55	+38	+12	− 4	−10	−17	− 9	− 4
1862	−12	−29	−49	−69	−71	−45	+11	+58	+91	+99	+72	+61	+26	+20	+18	− 8	+ 2	−14	−33	−19	−36	−31	−19	−23
1863	− 7	+ 4	+ 1	− 9	−25	−31	+ 8	+43	+84	+109	+70	+29	−26	−70	−78	−67	−38	− 3	+23	+22	+12	− 9	−25	−17
1864	+58	+42	+21	−21	−36	−44	−46	−25	− 8	+ 6	+18	0	−17	−27	−21	+ 3	+26	+37	+18	−12	−22	−15	+11	+54
1865	−66	−44	−12	+ 9	+31	+22	− 8	−17	−13	+12	+59	+80	+89	+80	+54	+37	+29	+11	−17	−40	−69	−82	−76	−69
1866	−15	−12	− 4	− 7	−11	−34	−62	−65	−96	−71	−18	+39	+82	+83	+56	+21	− 5	− 9	+13	+38	+46	+43	+15	− 7
1867	−62	−68	−70	−73	−69	−50	−34	−19	− 3	+10	+34	+45	+55	+67	+68	+85	+88	+76	+56	+21	−10	−35	−53	−59
1868	− 3	+ 2	+14	+13	− 6	−37	−63	−70	−58	−29	+ 3	+32	+43	+52	+55	+45	+36	+ 8	− 6	− 7	−12	+ 4	− 3	−13
1869	+23	−11	−49	− 60	−77	−79	−77	−94	−91	−89	−73	−52	−31	−13	+11	+60	+98	+133	+128	+102	+77	+59	+60	+45
1870	+ 5	− 2	− 2	+ 5	+23	+34	+27	+11	−13	−34	−40	−42	−33	−17	− 7	− 1	+ 9	+18	+16	+18	+ 8	+ 1	+ 6	+10
1871	−47	−24	−12	+10	+37	+45	+53	+53	+36	+30	+32	+32	+45	+59	+53	+38	+16	−21	−47	−73	−88	−89	−79	−59
1872	+15	+32	+51	+53	+33	+24	− 1	+ 3	+20	+ 6	+13	− 7	−31	−30	−45	−42	−33	−34	−18	−12	− 3	+ 1	0	+ 5
1873	+ 5	−23	−42	−49	−29	+16	+42	+60	+60	+39	+31	+21	+11	−16	−34	−49	−49	−22	− 8	+ 5	+ 3	+ 5	+11	+12
		−64	−68	−108	−224	−300	−325	−298	−176	− 4	+144	+302	+344	+332	+304	+229	+199	+199	+159	+60	−36	−159	−218	−185	−107
																								(sum=4544)

It will be noticed that the sums 96820 and 93739 are not far different from those of the temperature inequalities given in Table III, a result which would appear to indicate that for the purposes of this inquiry it is better not to eliminate disturbances.

16. In a paper puplished in the "Proceedings of the Royal Society" (No. 192, 1879), by Balfour Stewart and Morisabro Hiraoka, certain evidence is given that variations of magnetic declination, or what we may call magnetic weather changes, occur later in point of time at Trevandrum, in India, than at Kew, the difference being 9·7 days.

We are thus led to ask whether in our present investigation the corresponding phases may not be somewhat later at Prague than at Kew. Taking Kew as our standard, and applying the test already fully described in Article 6 of our present communication, we obtain the following result:—

Table XII.—Algebraic sum of Kew and Prague Magnetic Inequalities.

Kew and Prague	(Prague 1 division to right)	=170005
,,	(together)	=178078
,,	(Prague 1 division to left)	=178501
,,	(Prague 2 divisions to left)	=174147
,,	(Prague 3 divisions to left)	=165505

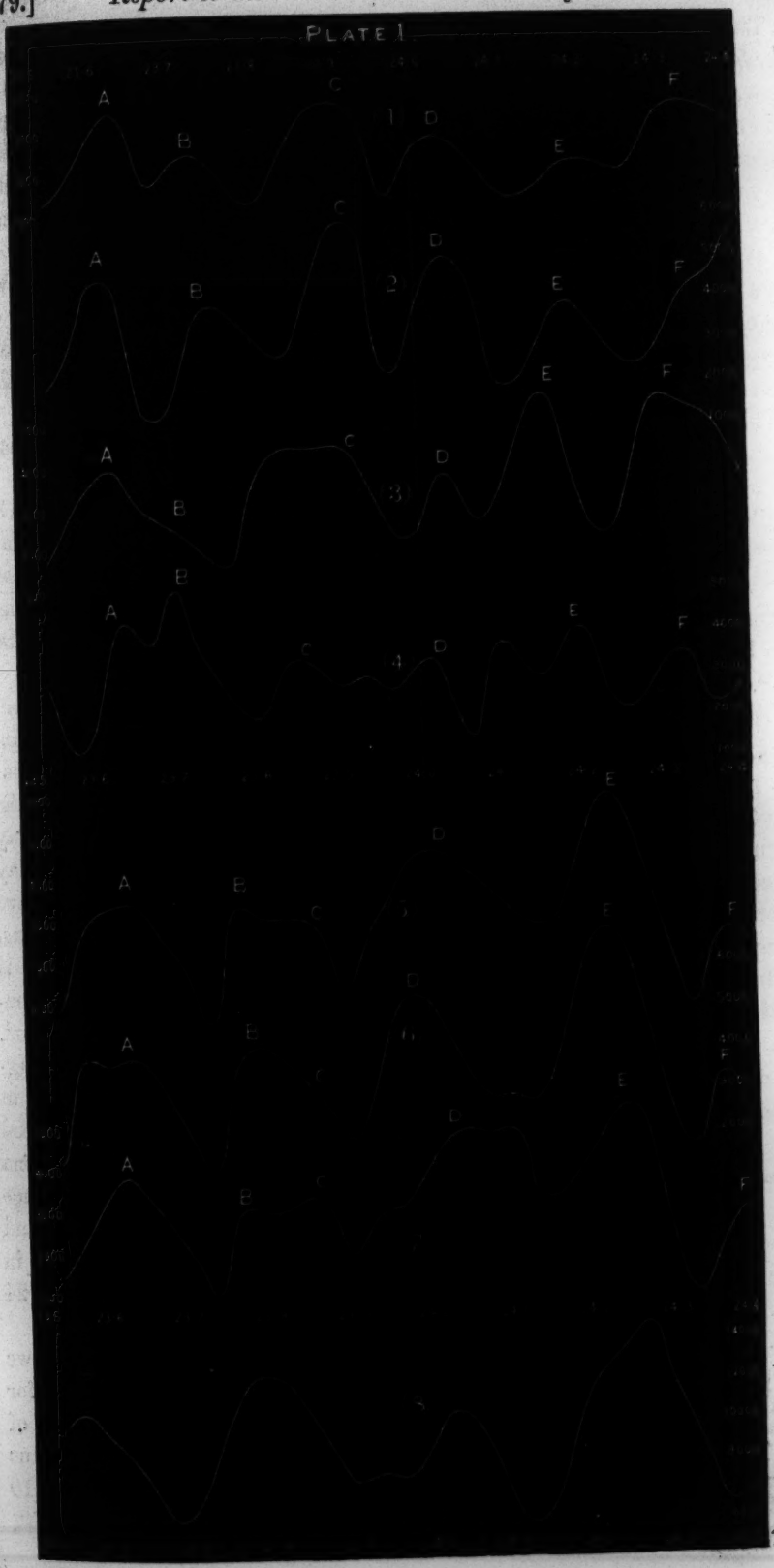
It would appear from this table that the phases of the various magnetic inequalities occur at Kew nearly one day before the advent of the corresponding phases at Prague.

In combining together the results of the two observatories, let us therefore push the Prague inequalities one division to the left.

17. When this is done, we obtain the following table:—

Table XIII.—In which Kew and Prague are added together as now described and the sums divided by 2.

Division from normal.	Magnitude of Inequality.	Division from normal.	Magnitude of Inequality.
−7·0	1123	0·0	4866
−6·5	3042	+0·5	4618
−6·0	3631	+1·0	4116
−5·5	3707	+1·5	3528
−5·0	2979	+2·0	3102
−4·5	2111	+2·5	3501
−4·0	849	+3·0	5459
−3·5	3590	+3·5	6229
−3·0	3304	+4·0	4663
−2·5	3289	+4·5	2651
−2·0	3017	+5·0	1189
−1·5	1690	+5·5	2944
−1·0	3168	+6·0	2586
−0·5	4298		


PLATE I
A
B
C
D
E
F
1
2
3
4

The results of Tables XI and XIII are exhibited in Plate 1, where fig. 5 denotes the mean curve of magnetic periods, fig. 6 the curve of Kew, and fig. 7 that of Prague. It will be noticed that these three curves are extremely like one another. It will also be noticed that the meteorological periods A, D, E, F, are well reproduced in the magnetic curves, while, however, the two peaks, B and C, appear to be squeezed together into a table-land. We think that this want of similarity of these two peaks is probably due to certain local influences which we have not been able to overcome, through the comparative paucity of our observations. Leaving out of account this table-land, we may now compare together the four magnetic inequalities A, D, E, F, with the corresponding meteorological ones.

This is done in Table XIV, which is precisely similar in construction to Table VIII.

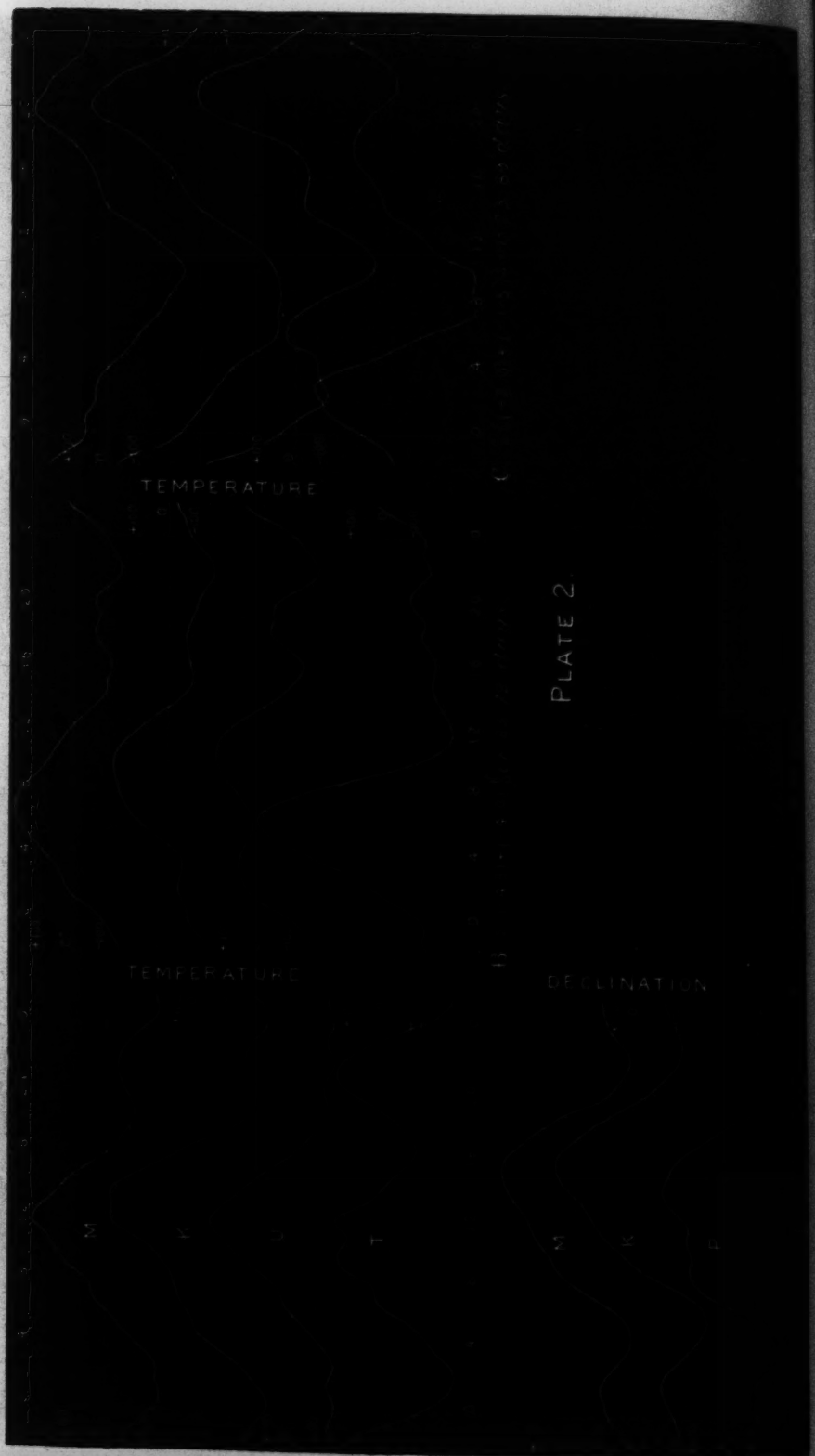
The results of this table are graphically represented in Plates 2 and 3. While there is a very striking likeness between the corresponding magnetic inequalities, there is also a very considerable likeness between the various corresponding magnetic and meteorological inequalities representing periods A, D, F. There is not, however, a strong likeness between those representing E.

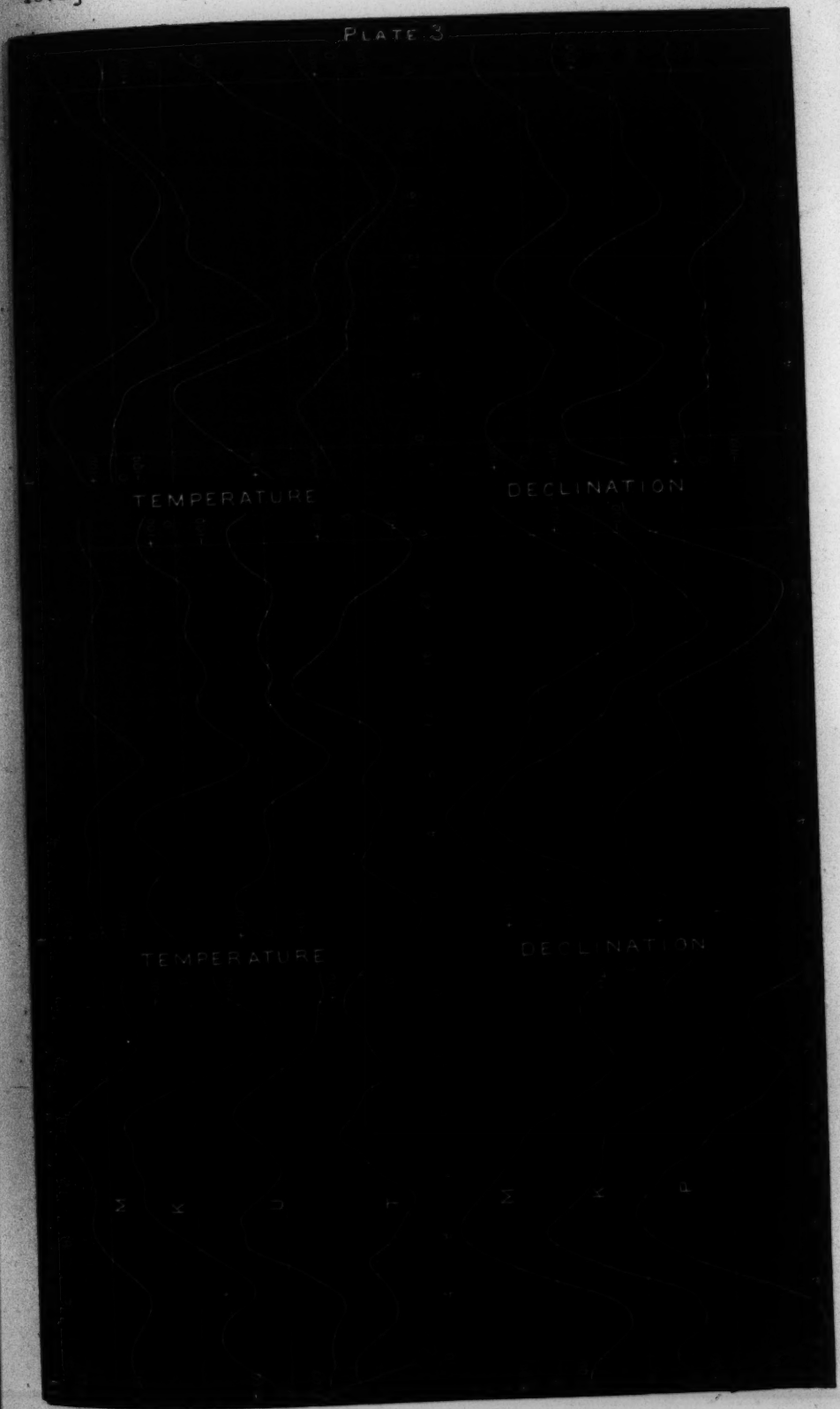
18. But here it is well to bear in mind that, while we should expect our method to indicate the various natural periods, whether in sun-spots, meteorology, or magnetism, it does not necessarily follow that the magnetic inequalities should be coincident in phase with the meteorological ones. For instance, it is not necessary that the 24-day magnetic inequality at Kew should coincide in phase with the 24-day meteorological inequality; for if it be true that in magnetism we have at once a 24-day inequality, simultaneously affecting all stations, influencing the earth as a magnet, in whatever way the influence is produced, and also something more allied to the meteorological inequalities, travelling from west to east, having likewise a 24-day period, then it is quite conceivable that the superposition of these two magnetical causes should produce a period also of 24 days, but differing in phase from the corresponding meteorological period. It will be noticed that we do not presume to assert that the magnetical phenomena are as we have just stated, but only that they may be so, and hence that a coincidence in phase of the two curves, magnetical and meteorological, is not a necessary consequence of the existence in these two phenomena of a common inequality, having a period of 24 days.

19. Owing to the present imperfect state of sun-spot recorsd, we have not been able to obtain trustworthy results for the 16 years for which we have discussed the other phenomena. The researches of Dr. De La Rue and his colleagues have, however, given us the means of exhibiting certain provisional results, extending throughout the 10

Table XIV.—In which are compared together the various Magnetic Inequalities representing A, D, E, F at the two Observatories Kew and Prague (K=Kew, P=Prague).

$A=\frac{(-6\cdot0)+(-5\cdot5)}{2}$	Mean ...	−109	− 92	− 79	−110	−159	−178	−162	−110	− 20	+ 51	+ 83	+122	+145	+180	+219	+233	+228	+176	+108	+ 23	− 86	−137	−171	−158
	K	−122	−100	− 77	− 94	−145	−193	−221	−210	−135	− 40	+ 35	+ 95	+141	+169	+215	+253	+265	+227	+167	+ 91	− 3	− 64	−116	−134
	P	− 95	− 83	− 81	−126	−172	−163	−102	− 9	+ 95	+141	+132	+149	+148	+190	+223	+213	+190	+125	+ 48	− 44	−169	−210	−226	−182
$D=\frac{(0\cdot0)+(+0\cdot5)}{2}$	Mean ...	−128	−191	−245	−238	−172	− 28	+116	+229	+305	+316	+288	+237	+195	+151	+117	+ 83	+ 12	− 72	−147	−208	−214	−178	−126	−104
	K	− 91	−123	−191	−192	−159	− 24	+138	+251	+327	+310	+246	+197	+154	+142	+138	+111	+ 48	− 38	−147	−231	−279	−263	−184	−134
	P	−165	−258	−300	−284	−184	− 31	+ 94	+207	+282	+321	+329	+278	+237	+160	+ 95	+ 54	− 23	−106	−147	−185	−148	− 92	− 68	− 73
$E=\frac{(+3\cdot0)+(+3\cdot5)}{2}$	Mean ...	+ 5	+ 84	+148	+224	+260	+314	+317	+266	+212	+122	+ 69	+ 52	+ 27	+ 31	− 10	−113	−194	−281	−331	−323	−313	−271	−197	−104
	K	− 78	− 4	+ 93	+227	+308	+371	+393	+344	+310	+223	+119	+ 63	− 11	− 36	− 60	−157	−230	−323	−375	−336	−292	−232	−169	−142
	P	+ 89	+173	+203	+221	+212	+257	+240	+188	+114	+ 21	+ 18	+ 41	+ 65	+ 98	+ 40	− 68	−158	−238	−286	−309	−333	−310	−224	− 66
$F=\frac{(+5\cdot0)+(+5\cdot5)}{2}$	Mean ...	+137	+151	+136	+ 26	− 42	− 59	− 81	− 25	+ 11	+ 46	+100	+ 99	+ 64	− 9	− 88	−140	−147	−108	− 71	− 52	− 24	− 24	+ 9	+ 90
	K	+177	+191	+184	+ 44	− 88	−127	−151	− 68	+ 14	+ 87	+152	+141	+ 88	+ 6	−102	−155	−156	−113	− 58	− 47	− 39	− 39	− 21	+ 88
	P	+ 97	+111	+ 88	+ 7	+ 4	+ 9	− 10	+ 18	+ 8	+ 6	+ 48	+ 57	+ 40	− 24	− 74	−124	−139	−102	− 84	− 57	− 10	− 8	+ 40	+ 92


TEMPERATURE
PLATE 2
TEMPERATURE
DECLINATION


PLATE 3
TEMPERATURE
DECLINATION
TEMPERATURE
DECLINATION

years from 1858 to 1867. These we have treated precisely in the method already indicated, that is to say, grouping them in periods of 24 days, and obtaining yearly results. The only difference in treatment is that we esteemed it superfluous to apply any equalisation to the sun-spot records, as the yearly results were sufficiently marked without this process.

This has furnished us with the following table, analogous to Table I (see next page):—

20. Applying our method to the results of Table XV, we obtain the following numbers:—

Table XVI.—Exhibiting the Results of our Method applied to Table XV.

Division from normal.	Magnitude of Sun-spot Inequality.	Division from normal.	Magnitude of Sun-spot Inequality.
−7·0	10014	0·0	9744
−6·5	10635	+0·5	10821
−6·0	9288	+1·0	9322
−5·5	7944	+1·5	6127
−5·0	5956	+2·0	5646
−4·5	6177	+2·5	8513
−4·0	9300	+3·0	12240
−3·5	12035	+3·5	13765
−3·0	12594	+4·0	15096
−2·5	11309	+4·5	12140
−2·0	9134	+5·0	9568
−1·5	7403	+5·5	6335
−1·0	7790	+6·0	7054
−0·5	7665		

The results of Table XVI are graphically represented in Plate 1, fig. 8. It will be observed that there is a very striking likeness between the solar inequalities and those indicated in meteorology and magnetism; but, as we have said before, owing to the imperfection of solar records, this can only be regarded as a provisional result.

We have not attempted in the meantime to discuss the individual solar inequalities or to represent them in the form of curves.

21. Let us now endeavour to bring together the various results of our present communication.

(1.) The temperature-ranges at Kew, Utrecht, and Toronto exhibit certain common periods (around 24 days).

(2.) Of the curves embodying those periods that of Kew is most like the mean, and that of Toronto least so.

(3.) Similar phases appear to occur at Toronto 8 days before they occur at Kew, and occur at Kew one day before they occur at Utrecht.

(4.) Correcting for these differences of phase, the individual in-

Table XV.—Unequalised Yearly Proportional Inequalities of Solar-Spotted Area corresponding to period 24·00 days.

Year	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1858	− 49	+ 28	+ 99	+130	+106	+ 65	+ 45	+ 9	+ 8	+ 41	+100	+ 52	− 22	− 14	− 42	−128	−181	−194	− 84	− 13	− 5	+ 96	− 2	− 45
1859	− 84	− 99	−131	−136	−149	−108	− 66	− 52	− 35	− 42	− 6	+ 28	+ 62	+105	+115	+153	+191	+193	+167	+ 96	+ 17	− 48	− 99	− 72
1860	+151	+ 57	− 28	− 4	− 30	− 77	−162	−255	−255	−232	−224	− 42	− 22	− 3	+ 76	+126	+150	+144	+ 78	+ 84	+100	+118	+136	+114
1861	−169	−140	−179	− 94	− 51	− 25	−106	− 52	− 76	−109	− 62	+157	+177	+183	+176	+155	+ 74	+ 94	+ 35	+ 3	+ 89	+ 58	+ 38	−176
1862	+ 1	− 18	− 37	− 13	+ 79	+ 80	− 34	− 55	− 71	− 78	−124	− 66	− 22	+ 65	+ 28	+ 2	+ 12	+ 63	+112	+165	+ 83	− 21	− 83	− 68
1863	−314	−287	−119	+ 38	+137	+ 76	+ 17	− 16	+ 25	+ 24	+ 48	+180	+148	+113	+ 20	0	+ 5	+ 17	− 21	+128	+ 73	− 55	− 94	−143
1864	+ 29	+ 80	+ 98	+ 63	+ 97	+139	+143	+136	+125	+ 86	+ 6	− 28	+ 66	+ 12	− 19	− 47	− 16	− 59	− 97	−130	−155	−210	−180	−139
1865	−391	−201	−320	−207	−111	− 62	+ 40	+116	+218	+255	+286	+214	+264	+420	+416	+246	+118	− 38	−111	−143	−190	−180	−277	−362
1866	+ 84	+339	− 37	−156	− 61	+ 61	+ 46	+ 30	− 35	− 55	− 78	− 84	−112	−154	−114	−106	− 62	− 39	− 17	+ 81	+125	+168	+114	+ 62
1867	+229	−102	−204	−255	−265	−279	−274	−258	−241	−224	−234	−202	−233	− 62	+ 58	+141	+201	+144	+149	+267	+363	+444	+402	+435
	−513	−343	−858	−634	−248	−130	−351	−397	−337	−334	−288	+209	+ 306	+665	+714	+542	+492	+325	+211	+538	+500	+370	− 45	−394
																								(sum=9744

equalities at Kew, Utrecht, and Toronto are very like the mean of the three and like each other.

(5.) Of these the Kew inequalities are most like the mean, and the Toronto least so.

(6.) The declination-ranges at Kew and Prague exhibit certain common periods which we may regard as the same, or very nearly the same, as the meteorological periods above indicated.

(7.) Similar magnetic phases appear to occur at Kew about one day before they occur at Prague.

(8.) Correcting for this difference of phase, the individual inequalities at Kew and Prague are very like the mean of the two, and like each other.

(9.) There is also a less striking likeness between the various magnetical and the corresponding meteorological inequalities.

(10.) Provisional sun-spot records appear to show certain solar inequalities very like the magnetic and meteorological inequalities in point of period.

22. In conclusion we would express our hope that steps will be taken to ascertain what information exists in the world available for these researches, and also to bring such information together for the purpose of a further and more complete discussion of the laws now indicated.

VI. "On the Solubility of Solids in Gases." By J. B. Hannay, F.R.S.E., F.C.S., and James Hogarth. Communicated by Professor G. G. Stokes, D.C.L., LL.D., Sec. R.S. Received October 8, 1879.

(Preliminary Notice.)

This investigation was undertaken in the hope that, by an examination of the conditions of liquid matter up to the "critical" point, sufficient knowledge might be gained to enable us to determine under what particular conditions liquids are dynamically comparable, in order that the microrheometrical method* (which the Royal Society has done one of us the honour of publishing in the "Philosophical Transactions") might be applied to determine their molecular mass and energy relations. It seemed that as the laws relating to gases and liquids merge at what was called by Baron Cagniard de la Tour† "l'état particulier," and by Dr. Andrews‡ the "critical point," an examination of matter up to the limit of the liquid state would be

* "On the Microrheometer," "Phil. Trans.," vol. clxx, p. 275.

† "Ann. Chim.," Series 2me, xxi, p. 127; xvii, p. 410.

‡ "Bakerian Lecture," "Phil. Trans. Roy. Soc.," 1869, p. 588.

likely to yield us much information. The time we have to devote to scientific work being very limited, we found that it was quite impossible to make much advance by using the apparatus devised by Dr. Andrews, as the time required to change from one liquid to another was more than we had at our disposal. We therefore devised a new apparatus, which will be described in a more lengthy communication, but which, we may state, can be opened, the liquid changed, and again closed for a new experiment, in about one minute.

The question as to the state of matter immediately beyond the critical point being considered by Dr. Andrews to be at that time incapable of receiving an answer, we imagined that some insight might be gained into its condition by dissolving in the liquid some solid substance whose fusing point was much above the critical point of the liquid, and noticing whether, on the latter passing its critical point and assuming the gaseous condition, the solid was precipitated or remained in solution. We found that the solid was not deposited but remained in solution, or rather in diffusion, in the atmosphere of vapour, even when the temperature was raised 130° above the critical point, and the gas was considerably expanded. When the side of a tube containing a strong gaseous solution of a solid is approached by a red hot iron, the part next the source of heat becomes coated with a crystalline deposit which slowly redissolves on allowing the local disturbance of temperature to disappear. Rarefaction seems to be the cause of this deposition, because if the temperature be raised equally and the volume retained at its original value, no deposition takes place. Those experiments have been done with such solvents as alcohol (ethyl and methyl), ether, carbon disulphide and tetrachloride, paraffins, and olefines, and such solids as sulphur, chlorides, bromides, and iodides of the metals, and organic substances such as chlorophyll and the aniline dyes. Some solutions show curious reactions at the critical point. Thus ethyl alcohol, or ether, deposits ferric chloride from solution just below the critical point, but re-dissolves it in the gas, when it has been raised 8° or 10° above that temperature.

It appeared to us to be of some importance to examine the spectroscopic appearances of solutions of solids when their liquid menstrua were passing to the gaseous state, but as all the substances we have yet been able to obtain in the two states give banded spectra with nebulous edges, we are only able to state that the substance does not show any appreciable change at the critical point of its solvent. Such was the case with anhydrous chloride of cobalt in absolute alcohol. It was suggested to us by Professor Stokes that the substance obtained by the decomposition of the green colouring matter of leaves by acids, and which yields a very fine absorption spectrum, might be useful for our purpose. We have prepared the substance according to the care-

ful directions so kindly furnished us by Professor Stokes, and find that it shows the phenomenon in a marked manner, whether dissolved in alcohol or ether. The compound is easily decomposed by heat under ordinary circumstances, and yet can be dissolved in gaseous menstrua, and raised to a temperature of 350° without suffering any decomposition, showing the same absorption spectrum at that elevated temperature as at 15°.

We considered that it would be most interesting to examine by this method a body such as sodium, which, besides being an element, yields in the gaseous state sharp absorption lines. An opportunity seemed to be afforded by the blue solution of sodium in liquefied ammonia, described by Gore,* but we found that, on raising the ammonia above its critical point, the sodium combined with some constituent of the gas, forming a white solid, and yielding a permanent gas, probably hydrogen.

There seems, in some cases, to be a slight shifting of the absorption bands towards the red, as the temperature rises, but we have as yet been able to make no accurate measurements.

When the solid is precipitated by suddenly reducing the pressure, it is crystalline, and may be brought down as a "snow" in the gas, or on the glass as a "frost," but it is always easily redissolved by the gas on increasing the pressure. These phenomena are seen to the best advantage by a solution of potassic iodide in absolute alcohol.

We have, then, the phenomenon of a solid with no measurable gaseous pressure, dissolving in a gas, and not being affected by the passage of its menstruum through the critical point to the liquid state, showing it to be a true case of gaseous solution of a solid.

VII. "On the Action of Nuclei in producing the Sudden Solidification of Supersaturated Solutions of Glauber's Salt." By Charles Tomlinson, F.R.S. Received October 6, 1879.

I propose now to state the general conclusions at which I have arrived since resuming the study of this subject two years ago.† The remarks that will be made refer to solutions of the typical salt, sodic sulphate, in the proportions of 6 of salt to 3 of water.

In my first two papers on supersaturated saline solutions,‡ a nucleus is defined as a body that has a stronger adhesion for the salt, or for the liquid of a solution, than subsists between the salt and the liquid.

* "Proc. Roy. Soc.," vol. xxi, p. 145.
† "Proc. Roy. Soc.," vol. xxvi, p. 523.
‡ "Phil. Trans.," 1868 and 1871.

Many examples are given in which oils, fixed and volatile, alcohol, ether, &c., spreading on the surface of the solution, lead to the separation of salt, and the solidification of the solution; while, if such liquids, instead of spreading, assume a lenticular form, they become inactive, and by shaking the flask with a circular motion, are dispersed through the solution in numerous globules, without any immediate nuclear action.

There are several circumstances which favour the action of these liquid nuclei, such as (1) chemically clean flasks and solutions, so as to maintain (2) the surface-tension of the solution as high as possible, in order to spread the oils; (3) bright and clear weather, with strong evaporative force. Under such conditions the oils, &c., usually form films with nuclear action.

But it has been shown that in closed flasks, with special precautions against the entrance of particles from the air, the oils, &c., may be made to spread on the surface of these solutions without any nuclear effect.

The negative results arrived at by M. Viollette, Mr. Liversidge, and other eminent observers, led them to conclude that oils, alcohol, ether, &c., as well as porous bodies, have no action whatever on these solutions, and that the only nucleus is a salt of the same kind as the one in solution, or isomeric therewith; and that, when the oils, &c., appear to act they really contain particles of such a salt; or when the flask is uncovered for the purpose of introducing the oil, &c., such a saline speck enters from the air, which is supposed to contain saline nuclei floating about in it.

The objections made to my mode of experimenting, as well as to my conclusions, were fair enough, since, if my experiments always contained a flaw, the conclusions drawn from them would be worthless. But the thousands of results, recorded in my note books, are sufficiently harmonious to satisfy me that my mode of experimenting is trustworthy; nevertheless, I have to account for the results and conclusions of other observers which stand in direct opposition to mine.

It was a long time before I could hit upon a right method of pursuing the inquiry. At length I was led to make a number of observations daily, during some months, on the action of a freshly-distilled volatile oil on solutions of sodic sulphate, and found that during certain winds the oil was active, and that, while other winds prevailed, it was passive.*

It was clear then that, during some winds, a force was present in the air which rendered the oil active; while, during other winds, the force being absent, the oil became passive. I succeeded in identifying this force with ozone; and the point now to be determined is,

* These observations were made at Highgate. Other observations by the sea-side, &c., are noticed in "Proc. Roy. Soc.," vol. xxix, p. 24.

what is the peculiar function of ozone in rendering oils, &c., active, as nuclei?

It is evident, from the behaviour of ozone on volatile oils exposed to its influence that its action is an oxidising one, diminishing their cohesion, just as rust weakens the cohesive force of the particles in a bar of iron.

The effect which ozone produces quickly on a volatile oil is produced slowly on the same oil by long keeping. Its cohesive force is so far weakened, if not destroyed, that a drop of it on the surface of a solution no longer tends to assume the lenticular form, or to disperse in globules through the solution. Under such circumstances, since oil adheres much more strongly to salt than to water, and the supersaturated solution, being a highly charged system, capable of yielding to a force that is exerted in the right direction,* such an oil is capable of adhering to, and separating, a portion of the salt, and the action, once begun, is propagated rapidly, until the whole solution is solid.

The action of a foreign nucleus introduced into the cold solution from without, is to determine the formation of the ten-atom or normal salt: but if a solid body be boiled up with the solution, and the covered flask be left to cool to temperatures about and below 40° F., the modified or seven-watered salt will be found clustered about the solid body. An explanation of this may be found in the behaviour of the salt while undergoing solution. Suppose the flask to contain six parts of the crystallised, not effloresced, salt and three of water. If the heat be applied too strongly, the salt gives up its water of crystallisation, and the anhydrous salt is thrown down, and produces violent bumpings of the flask. But if the heat be kept below the temperature of maximum solubility of the salt (about 93° F.) until the whole of it is dissolved, the solution may be raised to the boiling point and also be cooled down in covered vessels to about 40° F., without any separation of salt. The anhydrous salt now appears to be in solution,† and a solid body previously heated in the solution forms a portion of it, or rather a portion of the flask, and may be regarded as a prolongation of its sides. Under a proper reduction of temperature, crystals of the seven-atom salt will cluster about such a body, but it cannot be said to exert any active nuclear function. It was pointed out by Ziz, as long ago as the year 1809,

* See a paper "On the Function of the sides of the Vessel in maintaining the State of Supersaturation." "Proc. Roy. Soc.," vol. xxvii, p. 189.

† See "Chemical News," 3rd and 10th December, 1869. In Wüllner's experiments on the elasticity of steam, when sodic sulphate was added to the water, the diminution in the elasticity was found to be proportional to the quantity of *dry* salt in solution at temperatures from 10° to 100° C. At the point of maximum solubility of the salt no molecular change occurred, or it would have impressed itself on the curve which represented the elasticity of the steam.

that solid bodies, boiled up with the solution, are inactive; and I have found them to be so when put into the hot or warm solution, and left till cold; and that oils and other liquids, boiled or heated with the solution, become equally inactive. Several observers, who have more or less recently worked on the subject before us, have not attended to this condition, and hence, obtaining negative results, have declared that the oils, &c., have no nuclear action under any circumstances.

If, however, a nucleus from without be introduced into the cold solution, a nucleus that is capable of adhering to the salt and not to the water of the solution, a molecule of the anhydrous salt is thereby set free, and in the very act of separating, ten atoms of water enter into combination, and thus determine the solidification of the whole of the solution in the form of the normal salt.*

If the nucleus fail to catch one of these saline molecules, as it were in the nascent state, but disperses through the solution, its effect is to lessen the adhesion between the water and the salt, and a portion of the seven-atom salt is liberated (mostly at the bottom of the vessel, where the solution is richer in salt) provided the temperature be not too high: this effect is also produced by cold alone.

If an essential oil, which acts as a nucleus in determining the formation of the normal salt, be submitted to distillation, the distillate reassumes its cohesive force, and when added to the solution, is no longer active, seeing that whether in the lenticular form, or dispersed in globules, it has a separate existence of its own, the lens and each minute globule being bound up in their own surface tension, which prevents them from coming into contact with the salt. If, however, the drop of oil spread out into a film upon the surface of the solution, the cohesion is so far weakened that adhesion between it and the salt becomes possible, separation usually takes place, and large crystals of the normal salt mould themselves on the under surface of the film.

If such be the true explanation, it is clear that any other force which diminishes or destroys the cohesive force of the oil, must confer upon it the same nuclear property. Consequently, a freshly distilled essential oil, mineral naphtha, &c., which are inactive, become active by the addition of a little absolute alcohol, ether and similar liquids in which they are soluble.

The fixed oils which tend to become rancid through age are also active, and the stearine deposited by them is especially so. Newly refined fixed oils, which are inactive, become active on the addition of a little alkali; but the tendency of potash, soda, and ammonia is to

* Löwel noticed that when the transparent seven-atom salt is removed from the solution, it becomes opaque and hot in consequence of the fixation of three additional atoms of water. In such case the supersaturated mother liquor on the surface and entangled among the crystals, produces, in the absence of a nucleus, a crystal of the normal salt by evaporation, and this acts as a nucleus.

form soluble soaps with the oils, which mingle with the solution often without nuclear action. Lime and magnesia are more manageable. On mixing the oil with one of these, and adding a drop or two to the solution, then inclining the flask nearly horizontally, and rotating it slowly on its axis, these insoluble soaps adhere to the side and form smears, upon which masses of large crystals of the normal salt start into existence in a striking manner, and the solution immediately becomes solid, so that on restoring the flask to its erect position, a vertical wall appears, with a solid mass on one side and an apparently empty space on the other.

Some of the solid and semi-solid fats in their natural state, or as they leave the hands of the cook, are admirable nuclei. Such are suet, dripping, fat of bacon and ham, lard, butter, and some others. But in all cases a freshly cut surface acts most effectually, and a fat that sometimes does not act for hours if cut one way, will act instantly if cut at right angles to the former direction. Lard contains a good deal of entangled water; this should be driven off. The lard when cold sometimes acts better when smeared on a clean glass rod than in a small lump.

The action of solid porous nuclei, such as plaster of Paris, pumice, &c., in determining the sudden solidification of these solutions admits of easy explanation. When these bodies are moderately absorptive they act by separating water from the solution; but when, by exposure to heat, their absorptive power is considerably increased, they become immediately saturated not with water, but with a portion of the solution itself, and hence there is no separation of salt. When such an apparently inactive body is exposed to the open air, its absorptive power becomes somewhat diminished by access of moisture, and it is brought into a condition to absorb more water from the solution, instead of the solution itself. In the experiments of Mr. Liversidge and others the porous bodies were thoroughly heated and dried before they were brought into contact with the solution, and this will sufficiently explain the negative results obtained by them in their carefully conducted experiments.

The action of nuclei on the supersaturated solutions of other salts that have engaged my attention, such as those of alum, zincic and magnesic sulphates, and one or two others, admits of similar explanations; but there are peculiarities belonging to each which need not at present occupy the time of the Society.

The curious condition of supersaturation was first observed by Dr. Black, towards the end of the last century, and from that time to the present innumerable notes and memoirs have been written on its varied phenomena. Some observers who have treated the subject most elaborately have adopted methods which, by the action of heat and the exclusion of air, have tended to promote the cohesive or adhesive force of

the bodies examined by them, and obtaining only negative results, and always a certain result with a crystal of the salt, they have insisted that this is the only nucleus. Others, again, have sought for an explanation in some catalytic or other mysterious force; while a third set of observers have declared it to be a matter of uncertainty or hazard whether a foreign body acts as a nucleus or not. In reviewing the subject and repeating my experiments in various ways, I see no reason for withdrawing from the theory which I had the honour of submitting to the Society eleven years ago, namely, that the action of nuclei is simply mechanical, and is capable of being expressed by the familiar word adhesion.

VIII. "On Definite Integrals involving Elliptic Functions." By J. W. L. GLAISHER, M.A., F.R.S., Fellow of Trinity College, Cambridge. Received July 31, 1879.

§ 1. The chief object of this paper is to apply to definite integrals involving elliptic functions certain special methods which have been employed for the evaluation of integrals of a similar kind involving circular functions.

§ 2. One of the most elegant and direct investigations of the value of the integral

$$\int_0^{\frac{1}{2}\pi} \log \sin x \, dx = \tfrac{1}{2}\pi \log (\tfrac{1}{2})$$

is afforded by the product

$$\sin\frac{\pi}{n}\sin\frac{2\pi}{n}\ldots\sin\frac{(n-1)\pi}{2n} = \sqrt{n}\,.\,2^{-\frac{1}{2}(n-1)},$$

for, taking the logarithm of both sides of this equation, and writing $\frac{\pi}{n}=h$,

$$h(\log\sin h+\log\sin 2h\ldots+\log\sin\tfrac{1}{2}\pi)=\pi\lim_{n=\infty}\frac{\log\{\sqrt{n}\,.\,2^{-\frac{1}{2}(n-1)}\}}{n}$$
$$=-\tfrac{1}{2}\pi\log 2.$$

The same principle also gives the value of the integral

$$\int_0^{\pi}\log(1-2a\cos x+a^2)\,dx \quad \ldots\ldots\ldots \quad (1),$$

which $=0$ or $2\pi\log a$ according as $a<$ or >1, and it is easy to see in general that if

$$\phi\left(a+\frac{b}{n}\right)\phi\left(a+\frac{2b}{n}\right)\ldots\phi\left(a+\frac{pnb}{n}\right)=\psi(n),$$

then $$\int_a^{a+pb}\log\phi(x)dx=b\lim_{n=\infty}\frac{\log\psi(n)}{n}=b\frac{\psi'(\infty)}{\psi(\infty)}.$$

For example, from the product

$$\Gamma(a)\Gamma\left(a+\frac{1}{n}\right)\ldots\Gamma\left(a+\frac{n-1}{n}\right)=\Gamma(na)\,.\,(2\pi)^{\frac{1}{2}(n-1)}n^{\frac{1}{2}-na},$$

since, when n is great,

$$\Gamma(na)=(2\pi)^{\frac{1}{2}}(na)^{na-\frac{1}{2}}e^{-na},$$

we derive the evaluation,

$$\int_a^{a+1}\log\Gamma(x)dx=\tfrac{1}{2}\log(2\pi)+a(\log a-1).$$

The number of products that yield integrals of interest is not great, and those just noticed are all that I remember to have seen applied to this purpose. The transformation formulæ in elliptic functions lead, however, in this manner, immediately to definite integrals, as will appear in the next section.

§ 3. Writing sn, cn, dn in place of sin am, cos am, Δ am, we have ("Fundamenta Nova," p. 46):—

$$\{\mathrm{sn}\,2\omega\,\mathrm{sn}\,4\omega\ldots\mathrm{sn}\,(n-1)\omega\}^2=\frac{(-1)^{\frac{1}{2}(n-1)}}{\mathrm{M}}\left(\frac{\lambda}{k^n}\right)^{\frac{1}{4}}\quad..\quad(2).$$

$$\{\mathrm{cn}\,2\omega\,\mathrm{cn}\,4\omega\ldots\mathrm{cn}\,(n-1)\omega\}^2=\left(\frac{\lambda k'^n}{\lambda' k^n}\right)^{\frac{1}{4}}\quad.\quad.\quad.\quad.\quad.\quad(3).$$

$$\{\mathrm{dn}\,2\omega\,\mathrm{dn}\,4\omega\ldots\mathrm{dn}\,(n-1)\omega\}^2=\left(\frac{k'^n}{\lambda'}\right)^{\frac{1}{4}}.\quad.\quad.\quad.\quad.\quad.\quad.\quad(4).$$

In the case of the first real transformation (n being an uneven prime)

$$\omega=\frac{\mathrm{K}}{n},\qquad\Lambda=\frac{\mathrm{K}}{n\mathrm{M}},\qquad\Lambda'=\frac{\mathrm{K}'}{\mathrm{M}}.$$

Thus, when n is infinite,

$$\Lambda=\tfrac{1}{2}\pi,\qquad\mathrm{M}=\frac{2\mathrm{K}}{n\pi},\qquad\Lambda'=\tfrac{1}{2}n\frac{\pi\mathrm{K}'}{\mathrm{K}},$$

but $\Lambda'=\log\frac{4}{\lambda}$, so that $\qquad\lambda=e^{-\frac{1}{2}n\frac{\pi\mathrm{K}'}{\mathrm{K}}}$,

and we therefore have

$$\log\left(\frac{\lambda}{k^n}\right)^{\frac{1}{4}}=-\tfrac{1}{4}n\frac{\pi\mathrm{K}'}{\mathrm{K}}-\tfrac{1}{2}n\log k,$$

$$\log\left(\frac{\lambda k'^n}{\lambda' k^n}\right)^{\frac{1}{4}}=-\tfrac{1}{4}n\frac{\pi\mathrm{K}'}{\mathrm{K}}+\tfrac{1}{2}n\log\left(\frac{k'}{k}\right)-\tfrac{1}{2}\log(\tfrac{1}{2}\pi),$$

$$\log\left(\frac{k'^n}{\lambda'}\right)^{\frac{1}{4}}=\tfrac{1}{2}n\log k'-\tfrac{1}{2}\log(\tfrac{1}{2}\pi),$$

whence the products (2), (3), (4), give

$$\int_0^{\mathrm{K}} \log \operatorname{sn} x dx = -\tfrac{1}{4}\pi \mathrm{K}' - \tfrac{1}{2}\mathrm{K} \log k \quad . \quad . \quad . \quad . \quad (5),$$

$$\int_0^{\mathrm{K}} \log \operatorname{cn} x dx = -\tfrac{1}{4}\pi \mathrm{K}' + \tfrac{1}{2}\mathrm{K} \log\left(\frac{k'}{k}\right) . \quad . \quad . \quad . \quad (6),$$

$$\int_0^{\mathrm{K}} \log \operatorname{dn} x dx = \tfrac{1}{2}\mathrm{K} \log k' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

which are the analogues in elliptic functions of the integrals

$$\int_0^{\frac{1}{2}\pi} \log \sin x dx = \tfrac{1}{2}\pi \log\left(\tfrac{1}{2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8),$$

$$\int_0^{\frac{1}{2}\pi} \log \cos x dx = \tfrac{1}{2}\pi \log\left(\tfrac{1}{2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

The other formulæ, such as, *ex. gr.*,

$$\operatorname{sn} u \operatorname{sn}\{u+4\omega\} \ldots \operatorname{sn}\{u+4(n-1)\omega\} = \left(\frac{\lambda}{k^n}\right)^{\frac{1}{4}} \operatorname{sn}\left(\frac{u}{\mathrm{M}}, \lambda\right),$$

do not lead to new integrals, for this product gives

$$\int_0^{4\mathrm{K}} \log \operatorname{sn}(u+x)\, dx = -\pi \mathrm{K}' - 2\mathrm{K} \log k,$$

in which the sign of $\operatorname{sn}(u+x)$ when negative is to be changed, so that the quantity under the integral sign should be written $\frac{1}{2}\log \operatorname{sn}^2(u+x)$. This result, however, may be readily deduced from (5).

The remaining formulæ on page 51 of the "Fundamenta Nova" only produce the equation

$$\int_0^{\mathrm{K}} \tfrac{1}{2} \log\left(1 - \frac{\operatorname{sn}^2 u}{\operatorname{sn}^2 x}\right)^2 dx = \int_0^{\mathrm{K}} \log\left(1 - k^2 \operatorname{sn}^2 u \operatorname{sn}^2 x\right) dx,$$

which may be otherwise deduced from the formulæ (44) and (45) of § 10.

§ 4. In the second real transformation

$$\omega = \frac{i\mathrm{K}'}{n}, \qquad \lambda_1 = \tfrac{1}{2}\pi, \qquad \lambda_1' = e^{-\frac{1}{2}n\frac{\pi \mathrm{K}}{\mathrm{K}'}},$$

so that we find

$$\int_0^{\mathrm{K}'} \tfrac{1}{4} \log \operatorname{sn}^4 ix dx = -\tfrac{1}{2}\mathrm{K}' \log k \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

$$\int_0^{\mathrm{K}'} \log \operatorname{cn} ix dx = \tfrac{1}{4}\pi \mathrm{K} + \tfrac{1}{2}\mathrm{K}' \log\left(\frac{k'}{k}\right) \quad . \quad . \quad . \quad . \quad (11),$$

$$\int_0^{\mathrm{K}'} \log \operatorname{dn} ix dx = \tfrac{1}{4}\pi \mathrm{K} + \tfrac{1}{2}\mathrm{K}' \log k' \quad . \quad . \quad . \quad . \quad . \quad (12),$$

but if, in virtue of the formulæ

$$\operatorname{sn} ix = i\frac{\operatorname{sn}(x,k')}{\operatorname{cn}(x,k')},$$

$$\operatorname{cn} ix = \frac{1}{\operatorname{cn}(x,k')},$$

$$\operatorname{dn} ix = \frac{\operatorname{dn}(x,k')}{\operatorname{cn}(x,k')},$$

these equations be written in a real form, they only reproduce (5), (6), and (7), as might have been anticipated *à priori*.

§ 5. The double products ("Fundamenta Nova," p. 66)

$$\Pi \operatorname{sn}^2 \frac{2m\mathrm{K}+2m'i\mathrm{K}'}{n} = \frac{(-)^{\frac{1}{2}(n-1)}n}{k^{\frac{1}{2}(n^2-1)}},$$

$$\Pi \operatorname{cn}^2 \frac{2m\mathrm{K}+2m'i\mathrm{K}'}{n} = \left(\frac{k'}{k}\right)^{\frac{1}{2}(n^2-1)},$$

$$\Pi \operatorname{dn}^2 \frac{2m\mathrm{K}+2m'i\mathrm{K}'}{n} = k'^{\frac{1}{2}(n^2-1)},$$

give rise to the remarkable integrals

$$\int_0^{\mathrm{K}}\int_{-\mathrm{K}'}^{\mathrm{K}'} \log \operatorname{sn}(x+iy)\,dx\,dy = -\mathrm{K}\mathrm{K}'\log k \quad \ldots \quad (13),$$

$$\int_0^{\mathrm{K}}\int_{-\mathrm{K}'}^{\mathrm{K}'} \log \operatorname{cn}(x+iy)\,dx\,dy = \mathrm{K}\mathrm{K}'\log\left(\frac{k'}{k}\right) \quad \ldots \quad (14),$$

$$\int_0^{\mathrm{K}}\int_{-\mathrm{K}'}^{\mathrm{K}'} \log \operatorname{dn}(x+iy)\,dx\,dy = \mathrm{K}\mathrm{K}'\log k' \quad \ldots \quad (15).$$

These may of course also be written

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}'} \log\{\operatorname{sn}(x+iy)\operatorname{sn}(x-iy)\}\,dx\,dy = -\mathrm{K}\mathrm{K}'\log k\,;\ \&c.$$

§ 6. Using the values of the three integrals in (5), (6), (7), and observing that

$$\int_0^{\mathrm{K}} \log \operatorname{sn} 2x\,dx = \tfrac{1}{2}\int_0^{2\mathrm{K}} \log \operatorname{sn} x\,dx,$$

$$= \tfrac{1}{2}\int_0^{\mathrm{K}} \log \operatorname{sn} x\,dx + \tfrac{1}{2}\int_{\mathrm{K}}^{2\mathrm{K}} \log \operatorname{sn} x\,dx,$$

$$= \tfrac{1}{2}\int_0^{\mathrm{K}} \log \operatorname{sn} x\,dx + \tfrac{1}{2}\int_0^{\mathrm{K}} \log \operatorname{sn}(2\mathrm{K}-x)\,dx,$$

$$= \int_0^{\mathrm{K}} \log \operatorname{sn} x\,dx,$$

we find, by means of the formula

$$\operatorname{sn} 2x = \frac{2 \operatorname{sn} x \operatorname{cn} x \operatorname{dn} x}{1 - k^2 \operatorname{sn}^4 x},$$

that $$\int_0^{\mathrm{K}} \log (1 - k^2 \operatorname{sn}^4 x) dx = -\tfrac{1}{4}\pi \mathrm{K}' + \mathrm{K} \log \left(\frac{2k'}{k^{\frac{1}{2}}}\right) \quad . \quad . \quad (16).$$

Similarly

$$\int_0^{\mathrm{K}} \tfrac{1}{2} \log \operatorname{cn}^2 2x dx = \int_0^{\mathrm{K}} \log \operatorname{cn} x dx,$$

$$\int_0^{\mathrm{K}} \log \operatorname{dn} 2x dx = \int_0^{\mathrm{K}} \log \operatorname{dn} x dx,$$

whence, from the formulæ

$$\operatorname{cn} 2x = \frac{1 - 2 \operatorname{sn}^2 x + k^2 \operatorname{sn}^4 x}{1 - k^2 \operatorname{sn}^4 x},$$

$$\operatorname{dn} 2x = \frac{1 - 2k^2 \operatorname{sn}^2 x + k^2 \operatorname{sn}^4 x}{1 - k^2 \operatorname{sn}^4 x},$$

by the use of (16), we find

$$\int_0^{\mathrm{K}} \tfrac{1}{2} \log (1 - 2 \operatorname{sn}^2 x + k^2 \operatorname{sn}^4 x)^2 dx = -\tfrac{1}{2}\pi \mathrm{K}' + \mathrm{K} \log \left(\frac{2k'^{\frac{3}{2}}}{k}\right) \quad . \quad . \quad (17).$$

$$\int_0^{\mathrm{K}} \log (1 - 2k^2 \operatorname{sn}^2 x + k^2 \operatorname{sn}^4 x) dx = -\tfrac{1}{4}\pi \mathrm{K}' + \mathrm{K} \log \left(\frac{2k'^{\frac{3}{2}}}{k^{\frac{1}{2}}}\right) \quad . \quad . \quad (18).$$

These two integrals may also be put respectively in the forms

$$\int_0^{\mathrm{K}} \tfrac{1}{2} \log (\operatorname{cn}^2 x - \operatorname{sn}^2 x \operatorname{dn}^2 x)^2 dx \quad . \quad . \quad . \quad . \quad . \quad . \quad (19),$$

$$\int_0^{\mathrm{K}} \log (\operatorname{dn}^2 x - k^2 \operatorname{sn}^2 x \operatorname{cn}^2 x) dx \quad . \quad . \quad . \quad . \quad . \quad . \quad (20),$$

the former expression is written $\frac{1}{2} \log (\ )^2$, as the quantity in brackets is negative from $x = \frac{1}{2}\mathrm{K}$ to $x = \mathrm{K}$.

§ 7. Using the formulæ

$$\begin{aligned}
1 - \operatorname{cn} 2x &= 2 \operatorname{sn}^2 x \operatorname{dn}^2 x && \div (1 - k^2 \operatorname{sn}^4 x), \\
1 + \operatorname{cn} 2x &= 2 \operatorname{cn}^2 x && \div (1 - k^2 \operatorname{sn}^4 x), \\
1 - \operatorname{dn} 2x &= 2k^2 \operatorname{sn}^2 x \operatorname{cn}^2 x && \div (1 - k^2 \operatorname{sn}^4 x), \\
1 + \operatorname{dn} 2x &= 2 \operatorname{dn}^2 x && \div (1 - k^2 \operatorname{sn}^4 x), \\
\operatorname{cn}^2 x &= (\operatorname{dn} 2x + \operatorname{cn} 2x) && \div (1 + \operatorname{dn} 2x), \\
\operatorname{dn}^2 x &= (k'^2 + \operatorname{dn} 2x + k^2 \operatorname{cn} 2x) && \div (1 + \operatorname{dn} 2x),
\end{aligned}$$

and, observing that

$$\int_0^{\mathrm{K}} \phi(2x) dx = \tfrac{1}{2} \int_0^{2\mathrm{K}} \phi(x) dx,$$

we have

$$\int_0^{2K} \log(1 \pm \operatorname{cn} x)\,dx = -\tfrac{1}{2}\pi K' - K \log k \quad . \quad . \quad . \quad . \quad . \quad . \quad (21),$$

$$\int_0^{2K} \log(1 - \operatorname{dn} x)\,dx = -\tfrac{3}{2}\pi K' + K \log k \quad . \quad . \quad . \quad . \quad . \quad (22),$$

$$\int_0^{2K} \log(1 + \operatorname{dn} x)\,dx = \tfrac{1}{2}\pi K' + K \log k \quad . \quad . \quad . \quad . \quad . \quad (23),$$

$$\int_0^{2K} \log(\operatorname{dn} x + \operatorname{cn} x)\,dx = -\tfrac{1}{2}\pi K' + K \log\left(\frac{k'^2}{k}\right) \quad . \quad . \quad . \quad (24),$$

$$\int_0^{2K} \log(k'^2 + \operatorname{dn} x + k^2 \operatorname{cn} x)\,dx = \tfrac{1}{2}\pi K' + K \log(kk'^2) \quad . \quad (25).$$

Since $\operatorname{sn}(2K-u) = \operatorname{sn} u$, $\operatorname{dn}(2K-u) = \operatorname{dn} u$, $\operatorname{cn}(2K-u) = -\operatorname{cn} u$, it follows that

$$\left.\begin{aligned} \int_0^{2K} \phi(\operatorname{sn} x)\,dx &= 2\int_0^{K} \phi(\operatorname{sn} x)\,dx \quad . \quad . \quad . \quad . \quad . \\ \int_0^{2K} \phi(\operatorname{dn} x)\,dx &= 2\int_0^{K} \phi(\operatorname{dn} x)\,dx \quad . \quad . \quad . \quad . \quad . \\ \int_0^{2K} \phi(\operatorname{cn} x)\,dx &= \int_0^{K} \{\phi(\operatorname{cn} x) + \phi(-\operatorname{cn} x)\}\,dx \end{aligned}\right\} \quad . \quad (26),$$

and therefore from (22) and (23) we deduce that

$$\int_0^{K} \log(1 - \operatorname{dn} x)\,dx = -\tfrac{3}{4}\pi K' + \tfrac{1}{2} K \log k \quad . \quad . \quad . \quad (27),$$

$$\int_0^{K} \log(1 + \operatorname{dn} x)\,dx = \tfrac{1}{4}\pi K' + \tfrac{1}{2} K \log k \quad . \quad . \quad . \quad (28).$$

But (21) only gives the value of

$$\int_0^{K} \{\log(1 - \operatorname{cn} x) + \log(1 + \operatorname{cn} x)\}\,dx,$$

and is, therefore, equivalent to (5), while (24) and (25) are merely transformations of (5) and (28).

We also see that in (24) and (25) the integrals may be written

$$\int_0^{2K} \log(\operatorname{dn} x - \operatorname{cn} x)\,dx \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29),$$

$$\int_0^{2K} \log(k'^2 + \operatorname{dn} x - k^2 \operatorname{cn} x)\,dx \quad . \quad . \quad . \quad . \quad (30),$$

Applying (26) to (17) and (18) in the forms (19) and (20), we see that

$$\int_0^{2K} \tfrac{1}{2} \log(\operatorname{cn} x \pm \operatorname{sn} x \operatorname{dn} x)^2\,dx = -\tfrac{1}{2}\pi K' + K \log\left(\frac{2k'^{\frac{3}{2}}}{k}\right) \quad . \quad . \quad . \quad (31),$$

$$\int_0^{2K} \log(\operatorname{dn} x \pm k \operatorname{sn} x \operatorname{cn} x)\,dx = -\tfrac{1}{4}\pi K' + K \log\left(\frac{2k'^{\frac{3}{2}}}{k^4}\right) \quad . \quad . \quad . \quad (32).$$

§ 8. Since

$$\int_0^{\mathrm{K}} \phi(x)dx = \int_0^{\mathrm{K}} \phi(\mathrm{K}-x)dx \quad . \quad . \quad . \quad . \quad . \quad . \quad (33),$$

it follows from (27) and (28) that

$$\int_0^{\mathrm{K}} \log(\mathrm{dn}\, x - k')dx = -\tfrac{3}{4}\pi\mathrm{K}' + \tfrac{1}{2}\mathrm{K}\log(kk') \quad . \quad . \quad . \quad (34),$$

$$\int_0^{\mathrm{K}} \log(\mathrm{dn}\, x + k')dx = \tfrac{1}{4}\pi\mathrm{K}' + \tfrac{1}{2}\mathrm{K}\log(kk') \quad . \quad . \quad . \quad (35).$$

It may be remarked that the transformation (33) applied to the functions sn, cn, dn does not suffice to give the values of the integrals in (5) and (6), although we thus immediately obtain (7); for

$$\int_0^{\mathrm{K}} \log \mathrm{dn}\, x dx = \int_0^{\mathrm{K}} \log \mathrm{dn}\,(\mathrm{K}-x)dx = \int_0^{\mathrm{K}} \log k' dx - \int_0^{\mathrm{K}} \log \mathrm{dn}\, x dx,$$

giving

$$\int_0^{\mathrm{K}} \log \mathrm{dn}\, x dx = \tfrac{1}{2}\mathrm{K}\log k';$$

but

$$\int_0^{\mathrm{K}} \log \mathrm{sn}\, x dx = \int_0^{\mathrm{K}} \log \mathrm{cn}\, x dx - \int_0^{\mathrm{K}} \log \mathrm{dn}\, x dx,$$

and

$$\int_0^{\mathrm{K}} \log \mathrm{cn}\, x dx = \int_0^{\mathrm{K}} \log k' dx + \int_0^{\mathrm{K}} \log \mathrm{sn}\, x dx - \int_0^{\mathrm{K}} \log \mathrm{dn}\, x dx,$$

only lead to one equation between the integrals of $\log \mathrm{sn}\, x$ and $\log \mathrm{cn}\, x$, viz.,

$$\int_0^{\mathrm{K}} \log \mathrm{cn}\, x dx - \int_0^{\mathrm{K}} \log \mathrm{sn}\, x dx = \tfrac{1}{2}\mathrm{K}\log k'.$$

§ 9. Putting $x=y$ in the formula

$$\Theta(x+y)\Theta(x-y) = \frac{\Theta^2 x \Theta^2 y}{\Theta^2 0}(1 - k^2 \mathrm{sn}^2 x \,\mathrm{sn}^2 y) \quad . \quad . \quad . \quad (36),$$

it becomes

$$\Theta(2x) = \frac{\Theta^4 x}{\Theta^3 0}(1 - k^2 \mathrm{sn}^4 x) \quad . \quad . \quad . \quad . \quad . \quad (37),$$

whence

$$\int_0^{\mathrm{K}} \log \Theta(2x)dx = 4\int_0^{\mathrm{K}} \log \Theta x dx - 3\int_0^{\mathrm{K}} \log \Theta 0 dx + \int_0^{\mathrm{K}} \log(1 - k^2 \mathrm{sn}^4 x)dx.$$

Now

$$\int_0^{\mathrm{K}} \log \Theta(2x)dx = \int_0^{\mathrm{K}} \log \Theta x dx,$$

so that the equation is

$$\int_0^{\mathrm{K}} \log \Theta x dx = \mathrm{K}\log \Theta 0 - \tfrac{1}{3}\int_0^{\mathrm{K}} \log(1 - k^2 \mathrm{sn}^4 x)dx,$$

and substituting the value of the last-written integral from (16) and

putting for $\Theta 0$ its value, viz. $\left(\frac{2k'\mathrm{K}}{\pi}\right)^{\frac{1}{2}}$, this gives

$$\int_0^{\mathrm{K}} \log \Theta x\,dx = \tfrac{1}{12}\pi\mathrm{K}' + \tfrac{1}{6}\mathrm{K}\log\left(\frac{2kk'\mathrm{K}^3}{\pi^3}\right) \quad . \quad . \quad . \quad . \quad (38).$$

Also $$\mathrm{H}x = \sqrt{k}\,.\,\mathrm{sn}\,x\Theta x,$$

and integrating the logarithm of this equation, using (5) and (38), we have

$$\int_0^{\mathrm{K}} \log \mathrm{H}x\,dx = -\tfrac{1}{6}\pi\mathrm{K}' + \tfrac{1}{6}\mathrm{K}\log\left(\frac{2kk'\mathrm{K}^3}{\pi^3}\right).$$

Replace x, y in (36) by mx, nx and the formula becomes

$$\Theta(m+n)x\Theta(m-n)x = \frac{\Theta^2(mx)\Theta^2(nx)}{\Theta^2 0}(1-k^2\,\mathrm{sn}^2\,mx\,\mathrm{sn}^2\,nx),$$

whence, since (p being any integer)

$$\int_0^{\mathrm{K}} \log \Theta(px)\,dx = \int_0^{\mathrm{K}} \log \Theta x\,dx,$$

we have

$$\int_0^{\mathrm{K}} \log (1-k^2\,\mathrm{sn}^2\,mx\,\mathrm{sn}^2\,nx)\,dx = 2\left(\mathrm{K}\log\Theta 0 - \int_0^{\mathrm{K}} \log\Theta x\,dx\right) \quad . \quad (39),$$

if m and n be different integers, and

$$= 3\left(\mathrm{K}\log\Theta 0 - \int_0^{\mathrm{K}} \log\Theta x\,dx\right) \quad . \quad (40),$$

if m and n be equal integers.

Thus we find

$$\int_0^{\mathrm{K}} \log (1-k^2\,\mathrm{sn}^2\,mx\,\mathrm{sn}^2\,nx)\,dx = -\tfrac{1}{6}\pi\mathrm{K}' + \tfrac{2}{3}\mathrm{K}\log\left(\frac{2k'}{k^{\frac{1}{2}}}\right) \quad . \quad (41).$$

if m and n be different integers, and

$$= -\tfrac{1}{4}\pi\mathrm{K}' + \mathrm{K}\log\left(\frac{2k'}{k^{\frac{1}{2}}}\right) \quad . \quad (42),$$

if m and n be equal integers.

The relation

$$\int_0^{\mathrm{K}} \log (1-k^2\,\mathrm{sn}^2\,mx\,\mathrm{sn}^2\,nx)\,dx = \tfrac{2}{3}\int_0^{\mathrm{K}} \log (1-k^2\,\mathrm{sn}^4\,mx)\,dx \quad . \quad . \quad (43)$$

is curious. The value of the latter integral is of course immediately derivable from (16).

§ 10. (*Lemma.*) If $\phi(x)$ be an even function of x, and if $\phi(x+2\mathrm{K}) = \phi(x)$, then

$$\int_0^{\mathrm{K}} \phi(x+y)\,dx + \int_0^{\mathrm{K}} \phi(x-y)\,dx = 2\int_0^{\mathrm{K}} \phi(x)\,dx,$$

for, if u denote the left hand member of this equation,

$$\frac{du}{dy}=\int_0^{\mathrm{K}}\frac{d}{dy}\phi(x+y)dx+\int_0^{\mathrm{K}}\frac{d}{dy}\phi(x-y)dx,$$

$$=\int_0^{\mathrm{K}}\frac{d}{dx}\phi(x+y)dx-\int_0^{\mathrm{K}}\frac{d}{dx}\phi(x-y)dx,$$

$$=\phi(\mathrm{K}+y)-\phi(y)-\phi(\mathrm{K}-y)+\phi(-y),$$

$$=0,$$

so that u is independent of y and the lemma is proved. Now from (36),

$$\int_0^{\mathrm{K}}\log\Theta(x+y)dx+\int_0^{\mathrm{K}}\log\Theta(x-y)dx=2\int_0^{\mathrm{K}}\log\Theta x dx+2\mathrm{K}\log\Theta y$$

$$-2\mathrm{K}\log\Theta 0+\int_0^{\mathrm{K}}\log(1-k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y)dx,$$

and since Θx satisfies the conditions supposed in the lemma, the left-hand member of this equation is equal to the first term on the right-hand side, so that we obtain the formula

$$\int_0^{\mathrm{K}}\log(1-k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y)dx=2\mathrm{K}\log\frac{\Theta 0}{\Theta y}=\mathrm{K}\log\left(\frac{2k'\mathrm{K}}{\pi}\right)-2\mathrm{K}\log\Theta y \quad (44),$$

which we may write in the form

$$\int_0^{\mathrm{K}}\log(1-a^2\,\mathrm{sn}^2\,x)dx=\mathrm{K}\log\left(\frac{2k'\mathrm{K}}{\pi}\right)-2\mathrm{K}\log\Theta\left(\mathrm{sn}^{-1}\frac{a}{k}\right).$$

In virtue of the formula

$$\mathrm{sn}\,(x+y)\,\mathrm{sn}\,(x-y)=\frac{\mathrm{sn}^2\,x-\mathrm{sn}^2\,y}{1-k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y},$$

$$\mathrm{cn}\,(x+y)\,\mathrm{cn}\,(x-y)=\frac{1-\mathrm{sn}^2\,x-\mathrm{sn}^2\,y+k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y}{1-k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y},$$

$$\mathrm{dn}\,(x+y)\,\mathrm{dn}\,(x-y)=\frac{1-k^2\,\mathrm{sn}^2\,x-k^2\,\mathrm{sn}^2\,y+k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y}{1-k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y},$$

we can deduce by means of (44), since $\mathrm{sn}^2\,x$, $\mathrm{cn}^2\,x$, and $\mathrm{dn}\,x$ each satisfy the conditions of the lemma, that

$$\int_0^{\mathrm{K}}\tfrac{1}{2}\log(\mathrm{sn}^2\,x-\mathrm{sn}^2 y)^2dx=-\tfrac{1}{2}\pi\mathrm{K}'+\mathrm{K}\log\left(\frac{2k'\mathrm{K}}{k\pi}\right)-2\mathrm{K}\log\Theta y \quad . \quad (45),$$

$$\int_0^{\mathrm{K}}\tfrac{1}{2}\log(1-\mathrm{sn}^2\,x-\mathrm{sn}^2\,y+k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y)^2dx$$

$$=-\tfrac{1}{2}\pi\mathrm{K}'+\mathrm{K}\log\left(\frac{2k'^2\mathrm{K}}{k\pi}\right)-2\mathrm{K}\log\Theta y \quad . \quad (46),$$

$$\int_0^{\mathrm{K}} \log\,(1-k^2\,\mathrm{sn}^2\,x-k^2\,\mathrm{sn}^2\,y+k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y)\,dx$$
$$=\mathrm{K}\log\left(\frac{2k'^2\mathrm{K}}{\pi}\right)-2\mathrm{K}\log\Theta y \quad . \quad (47).$$

The last two integrals may also be written

$$\int_0^{\mathrm{K}} \tfrac{1}{2}\log\,(\mathrm{cn}^2\,x-\mathrm{dn}^2\,x\,\mathrm{sn}^2\,y)^2\,dx,$$
$$\int_0^{\mathrm{K}} \log\,(\mathrm{dn}^2\,x-k^2\,\mathrm{cn}^2\,x\,\mathrm{sn}^2\,y)\,dx,$$

and if these be transformed by the substitution of $\mathrm{K}-x$ for x, they become respectively

$$\int_0^{\mathrm{K}} \tfrac{1}{2}\log\,(\mathrm{sn}^2\,x-\mathrm{sn}^2\,y)^2\,dx-2\int_0^{\mathrm{K}} \log\,\mathrm{dn}\,x\,dx+2\mathrm{K}\log k',$$
$$\int_0^{\mathrm{K}} \log\,(1-k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y)\,dx-2\int_0^{\mathrm{K}} \log\,\mathrm{dn}\,x\,dx+2\mathrm{K}\log k',$$

so that of the four integrals (44), (45), (46), (47), the pair (44), (47) are convertible one with another, and also the pair (45), (46), by the substitution of $\mathrm{K}-x$ for x.

Integrating (44), . . . (47) with regard to y between the limits K and 0, and using (38) we obtain the following evaluations:—

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}} \log\,(1-k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y)\,dx\,dy=-\tfrac{1}{6}\pi\mathrm{K}\mathrm{K}'+\tfrac{2}{3}\mathrm{K}^2\log\left(\frac{2k'}{k^{\frac{1}{2}}}\right) \quad . \quad . \quad (48),$$

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}} \tfrac{1}{2}\log\,(\mathrm{sn}^2\,x-\mathrm{sn}^2\,y)^2\,dx\,dy=-\tfrac{2}{3}\pi\mathrm{K}\mathrm{K}'+\tfrac{2}{3}\mathrm{K}^2\log\left(\frac{2k'}{k^2}\right) \quad . \quad . \quad (49),$$

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}} \tfrac{1}{2}\log(1-\mathrm{sn}^2x-\mathrm{sn}^2y+k^2\,\mathrm{sn}^2x\,\mathrm{sn}^2y)^2\,dx\,dy=-\tfrac{2}{3}\pi\mathrm{K}\mathrm{K}'+\tfrac{2}{3}\mathrm{K}^2\log\left(\frac{2k'^{\frac{1}{2}}}{k^2}\right)$$
$$\quad . \quad . \quad (50),$$

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}} \log\,(1-k^2\,\mathrm{sn}^2\,x-k^2\,\mathrm{sn}^2\,y+k^2\,\mathrm{sn}^2\,x\,\mathrm{sn}^2\,y)\,dx\,dy$$
$$=-\tfrac{1}{6}\pi\mathrm{K}\mathrm{K}'+\tfrac{2}{3}\mathrm{K}^2\log\left(\frac{2k'^{\frac{1}{2}}}{k^{\frac{1}{2}}}\right) \quad . \quad . \quad (51),$$

§ 11. The lemma at the beginning of the last section may be deduced directly from the definition of integration when y is real; for, in virtue of the equation $\phi(x+2\mathrm{K})=\phi(x)$, $\phi(x+y)+\phi(x-y)$ can always be reduced to the form $\phi(x+a)+\phi(x-a)$ where $a<\mathrm{K}$; and

$$\int_0^{\mathrm{K}}\phi(x+a)\,dx+\int_0^{\mathrm{K}}\phi(x-a)\,dx$$
$$=h\{\phi(a)+\phi(a+h)+\phi(a+2h)\ldots+\phi(a+\mathrm{K})$$
$$+\phi(-a)+\phi(-a+h)+\phi(-h)+\phi(0)+\phi(h)\ldots+\phi(\mathrm{K}-a)\}.$$

Since $\phi(\mathrm{K}+a-h)=\phi(2\mathrm{K}-\mathrm{K}-a+h)=\phi(\mathrm{K}+a+h)$, &c., and $\phi(-h)=\phi(h)$, &c., this

$$=h\{\phi(0)+\phi(h)\ldots+\phi(\mathrm{K}-a)+\phi(\mathrm{K}-a+h)\ldots+\phi(\mathrm{K})$$
$$+\phi(0)+\phi(h)\ldots+\phi(a)+\phi(a+h)\ldots+\phi(\mathrm{K})\}$$
$$=2\int_0^{\mathrm{K}}\phi(x)dx.$$

The lemma is thus evidently true for all real values of y, and from the proof in § 11 it would appear that in general it was probably true when y was imaginary or of the form $a+bi$. It is, however, certainly not true in the case of $y=2i\mathrm{K}'$, for

$$\Theta(x+2i\mathrm{K}')=-e^{\frac{\pi}{\mathrm{K}}(\mathrm{K}'-ix)}\Theta x.$$

so that $$\log\Theta(x+2i\mathrm{K}')+\log\Theta(x-2i\mathrm{K}')=\frac{2\pi\mathrm{K}'}{\mathrm{K}}+2\log\Theta x,$$

and therefore

$$\int_0^{\mathrm{K}}\log\Theta(x+2i\mathrm{K}')dx+\int_0^{\mathrm{K}}\log\Theta(x-2i\mathrm{K}')dx=2\pi\mathrm{K}'+2\int_0^{\mathrm{K}}\log\Theta x dx.$$

It is also evident that (44), ... (47) cannot be universally true, for the left-hand members of these equations remain unaltered when y is increased by $2i\mathrm{K}'$, which is not the case with the right-hand members, since Θ is not periodic with respect to $2i\mathrm{K}'$.

To determine the imaginary values of y for which the lemma and the theorems (44), ... (47) deduced from it are true, consider the expansion of $\log\Theta(x)$ in a series of cosines, viz.:

$$\log\Theta x=\mathrm{A}-\frac{2q}{1-q^2}\cos\frac{\pi x}{\mathrm{K}}-\tfrac{1}{2}\frac{2q^2}{1-q^4}\cos\frac{2\pi x}{\mathrm{K}}-\tfrac{1}{3}\frac{2q^3}{1-q^6}\cos\frac{3\pi x}{\mathrm{K}}-\text{ &c. (52),}$$

where $$\mathrm{A}=\tfrac{1}{12}\frac{\pi\mathrm{K}'}{\mathrm{K}}+\tfrac{1}{6}\log\left(\frac{2kk'\mathrm{K}^3}{\pi^3}\right).$$

From this we obtain (38), viz.:

$$\int_0^{\mathrm{K}}\log\Theta x dx=\mathrm{AK},$$

since $$\int_0^{\mathrm{K}}\cos\frac{n\pi x}{\mathrm{K}}dx=0.$$

Now $\log\Theta(x+a)+\log\Theta(x-a)$

$$=2\mathrm{A}-\Sigma\frac{1}{n}\frac{2q^n}{1-q^{2n}}\left\{\cos\frac{n\pi(x+a)}{\mathrm{K}}+\cos\frac{n\pi(x-a)}{\mathrm{K}}\right\}$$

$$=2\mathrm{A}-\Sigma\frac{1}{n}\frac{4q^n}{1-q^{2n}}\cos\frac{n\pi x}{\mathrm{K}}\cos\frac{n\pi a}{\mathrm{K}},$$

so that the lemma will necessarily hold good so long as the series

$$\Sigma \frac{1}{n} \frac{4q^n}{1-q^{2n}} \cos\frac{n\pi x}{\mathrm{K}} \cos\frac{n\pi a}{\mathrm{K}}$$

is convergent.

Let $a=mi\mathrm{K}'$, then

$$\cos\frac{n\pi a}{\mathrm{K}}=\tfrac{1}{2}(q^{mn}+q^{-mn})=\tfrac{1}{2}\frac{1+q^{2mn}}{q^{mn}},$$

and the series

$$=\Sigma \frac{2}{n} \frac{1+q^{2mn}}{1-q^{2n}} q^{n(1-m)} \cos\frac{n\pi x}{\mathrm{K}},$$

which is convergent when $m<1$.

It follows therefore that the lemma and the theorems (44), . . . (47) are true when y is of the form $a+bi$, where b lies between K' and $-\mathrm{K}'$, and a is unrestricted.

In the case of $b=\mathrm{K}'$ the series becomes neutral, but it is easy to see that the lemma and theorems are still true; for, transform (44) by putting $y=i\mathrm{K}'+z$, so that

$$k\,\mathrm{sn}\,y=\frac{1}{\mathrm{sn}\,z},$$

then $\log\Theta y=\log\Theta(i\mathrm{K}'+z)$

$$=\log\{ik^{\frac{1}{2}}e^{\frac{\pi}{4\mathrm{K}}(\mathrm{K}'-2iz)}\,\mathrm{sn}\,z\Theta z\}$$

$$=\tfrac{1}{2}\log k+\tfrac{1}{4}\frac{\pi\mathrm{K}'}{\mathrm{K}}+\log\mathrm{sn}\,z+\log\Theta z+\log i-\frac{i\pi z}{2\mathrm{K}}.$$

Thus we have

$$\int_0^{\mathrm{K}}\log(\mathrm{sn}^2 z-\mathrm{sn}^2 x)\,dx=-\tfrac{1}{2}\pi\mathrm{K}'+\mathrm{K}\log\left(\frac{2k'\mathrm{K}}{k\pi}\right)-2\mathrm{K}\log\Theta z+i\pi(z-\mathrm{K}).$$

The imaginary term $i\pi(z-\mathrm{K})$ is due to the fact that $\mathrm{sn}^2 z-\mathrm{sn}^2 x$ changes sign when $x=z$, so that we might expect the term $(\mathrm{K}-z)\log(-1)$ to appear in the equation. Writing, therefore, the integral in the real form

$$\int_0^{\mathrm{K}}\tfrac{1}{2}\log(\mathrm{sn}^2 z-\mathrm{sn}^2 x)^2,$$

and throwing away the imaginary term, we have (45). It may be observed that (46) and (47) are also connected by the same substitution of $y=i\mathrm{K}'+z$.

§ 12. In the formula (44), viz.:

$$\int_0^{\mathrm{K}}\log(1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2 y)\,dx=2\mathrm{K}\{\log\Theta 0-\log\Theta y\},$$

put y equal to $\frac{1}{2}\mathrm{K}$, $\frac{1}{2}\mathrm{K}+i\mathrm{K}'$, $\frac{1}{2}i\mathrm{K}'$, $\mathrm{K}+\frac{1}{2}i\mathrm{K}'$, $\frac{1}{2}\mathrm{K}-\frac{1}{2}i\mathrm{K}'$, $\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$ re-

spectively; we thus find, on substituting for $\mathrm{sn}^2 y$ its value in the different cases

$$\int_0^{\mathrm{K}} \log\{1-(1-k')\,\mathrm{sn}^2 x\}dx = 2\mathrm{K}\{\log\Theta 0 - \log\Theta(\tfrac{1}{2}\mathrm{K})\},$$

$$\int_0^{\mathrm{K}} \log\{1-(1+k')\,\mathrm{sn}^2 x\}dx = 2\mathrm{K}\{\log\Theta 0 - \log\Theta(\tfrac{1}{2}\mathrm{K}+i\mathrm{K}')\},$$

$$\int_0^{\mathrm{K}} \log\{1+k\,\mathrm{sn}^2 x\}dx = 2\mathrm{K}\{\log\Theta 0 - \log\Theta(\tfrac{1}{2}i\mathrm{K}')\},$$

$$\int_0^{\mathrm{K}} \log\{1-k\,\mathrm{sn}^2 x\}dx = 2\mathrm{K}\{\log\Theta 0 - \log\Theta(\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')\},$$

$$\int_0^{\mathrm{K}} \log\{1-k(k-ik')\,\mathrm{sn}^2 x\}dx = 2\mathrm{K}\{\log\Theta 0 - \log\Theta(\tfrac{1}{2}\mathrm{K}-\tfrac{1}{2}i\mathrm{K}')\},$$

$$\int_0^{\mathrm{K}} \log\{1-k(k+ik')\,\mathrm{sn}^2 x\}dx = 2\mathrm{K}\{\log\Theta 0 - \log\Theta(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')\}.$$

To obtain the values of the thetas, put $x=\frac{1}{2}\mathrm{K}$, $y=\frac{1}{2}\mathrm{K}$ in (36), and this equation gives

$$\Theta^4(\tfrac{1}{2}\mathrm{K}) = \frac{1+k'}{2k'}\Theta^3 0\Theta\mathrm{K},$$

whence

$$\Theta(\tfrac{1}{2}\mathrm{K}) = \frac{2^{\frac{1}{2}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}} k'^{\frac{1}{4}}(1+k')^{\frac{1}{2}} \quad \ldots\ldots\ldots \quad (53).$$

From this, by taking $x=\frac{1}{2}\mathrm{K}$ in the formula

$$\Theta(x+i\mathrm{K}') = ik^{\frac{1}{4}}q^{-\frac{1}{4}}e^{\frac{-i\pi x}{2\mathrm{K}}}\,\mathrm{sn}\,x\,\Theta x,$$

we deduce that

$$\Theta(\tfrac{1}{2}\mathrm{K}+i\mathrm{K}') = e^{\frac{\pi\mathrm{K}'}{4\mathrm{K}}}\frac{(1+i)\mathrm{K}^{\frac{1}{2}}}{2^{\frac{1}{4}}\pi^{\frac{1}{2}}} k'^{\frac{1}{4}}(1-k')^{\frac{1}{2}} \quad \ldots\ldots \quad (54).$$

The value of $\Theta(\frac{1}{2}i\mathrm{K}')$ may be deduced from (53) by putting $x=\frac{1}{2}\mathrm{K}$ in the formula

$$\Theta x = \left(\frac{k'\mathrm{K}}{k\mathrm{K}'}\right)^{\frac{1}{2}} e^{-\frac{\pi x^2}{4\mathrm{K}\mathrm{K}'}}\frac{1}{\mathrm{cn}\,x}\Theta(ix, k'),$$

and changing the modulus from k to k': it is thus found that

$$\Theta(\tfrac{1}{2}i\mathrm{K}') = e^{\frac{\pi\mathrm{K}'}{16\mathrm{K}}}\frac{2^{\frac{1}{2}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}} k^{\frac{1}{4}}(1-k)^{\frac{1}{2}} \quad \ldots\ldots \quad (55),$$

and thence that

$$\Theta(\mathrm{K}+\tfrac{1}{2}i\mathrm{K}') = e^{\frac{\pi\mathrm{K}'}{16\mathrm{K}}}\frac{2^{\frac{1}{2}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}} k^{\frac{1}{4}}(1+k)^{\frac{1}{2}}.$$

Finally, from (36) by putting $x=\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$, $y=\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$, we find that

$$\Theta^4(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')=\frac{\Theta^3 0\,\Theta(\mathrm{K}+i\mathrm{K}')}{2k'(k'-ik)}$$

$$=e^{-\frac{\pi\mathrm{K}'}{4\mathrm{K}}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(k'+ik) \quad . \quad . \quad . \quad . \quad . \quad . \quad (56).$$

Substituting these values in the integrals and reducing, we have

$$\int_0^{\mathrm{K}}\log\{1-(1-k')\,\mathrm{sn}^2 x\}\,dx=\tfrac{1}{2}\mathrm{K}\log\left\{\frac{2k'^{\frac{3}{2}}}{1+k'}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (57),$$

$$\int_0^{\mathrm{K}}\tfrac{1}{2}\log\{1-(1+k')\,\mathrm{sn}^2 x\}^2\,dx=-\tfrac{1}{2}\pi\mathrm{K}'+\tfrac{1}{2}\mathrm{K}\log\left\{\frac{2k'^{\frac{3}{2}}}{1-k'}\right\} \quad . \quad . \quad (58),$$

$$\int_0^{\mathrm{K}}\log\{1+k\,\mathrm{sn}^2 x\}\,dx=-\tfrac{1}{8}\pi\mathrm{K}'+\tfrac{1}{2}\mathrm{K}\log\left\{\frac{2(1+k)}{k^{\frac{1}{2}}}\right\} \quad . \quad . \quad . \quad . \quad (59),$$

$$\int_0^{\mathrm{K}}\log\{1-k\,\mathrm{sn}^2 x\}\,dx=-\tfrac{1}{8}\pi\mathrm{K}'+\tfrac{1}{2}\mathrm{K}\log\left\{\frac{2(1-k)}{k^{\frac{1}{2}}}\right\} \quad . \quad . \quad . \quad . \quad (60),$$

$$\int_0^{\mathrm{K}}\log\{1-k(k-ik')\,\mathrm{sn}^2 x\}\,dx=-\tfrac{1}{8}\pi\mathrm{K}'+\tfrac{1}{2}\mathrm{K}\log\left\{\frac{2k'^{\frac{3}{2}}(k'+ik)}{k^{\frac{1}{2}}}\right\} \quad (61),$$

$$\int_0^{\mathrm{K}}\log\{1-k(k+ik')\,\mathrm{sn}^2 x\}\,dx=-\tfrac{1}{8}\pi\mathrm{K}'+\tfrac{1}{2}\mathrm{K}\log\left\{\frac{2k'^{\frac{3}{2}}(k'-ik)}{k^{\frac{1}{2}}}\right\} \quad (62).$$

In (58) as the integral is so written that its value may be real, the term involving i that enters from (54) has been rejected.

By the addition of (57) and (58) we obtain (17), while (59) and (60) lead in a similar manner to (16), and (61) and (62) to (18). The reason why this happens is easily seen, for since $1-2\,\mathrm{sn}^2 x+k^2\,\mathrm{sn}^4 x$ is the numerator of $\mathrm{cn}\,2x$, we have

$$1-2\,\mathrm{sn}^2 x+k^2\,\mathrm{sn}^4 x=\left\{1-\frac{\mathrm{sn}^2 x}{\mathrm{sn}^2\tfrac{1}{2}\mathrm{K}}\right\}\left\{1-\frac{\mathrm{sn}^2 x}{\mathrm{sn}^2(\tfrac{1}{2}\mathrm{K}+i\mathrm{K}')}\right\},$$

which $$=\{1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2(\tfrac{1}{2}\mathrm{K}+i\mathrm{K}')\}\{1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2\tfrac{1}{2}\mathrm{K}\},$$

$$=\{1-(1+k')\,\mathrm{sn}^2 x\}\{1-(1-k')\,\mathrm{sn}^2 x\},$$

and, similarly

$$1-k^2\,\mathrm{sn}^4 x=\{1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2\tfrac{1}{2}i\mathrm{K}'\}\{1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2(\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')\}$$

$$=\{1+k\,\mathrm{sn}^2 x\}\{1-k\,\mathrm{sn}^2 x\},$$

and

$$1-2k^2\,\mathrm{sn}^2 x+k^2\,\mathrm{sn}^4 x$$

$$=\{1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2(\tfrac{1}{2}\mathrm{K}-\tfrac{1}{2}i\mathrm{K}')\}\{1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')\}$$

$$=\{1-k(k-ik')\,\mathrm{sn}^2 x\}\{1-k(k+ik')\,\mathrm{sn}^2 x\}.$$

§ 13. In any integral we may replace $\mathrm{sn}\,x$, $\mathrm{cn}\,x$, $\mathrm{dn}\,x$, by $\sin x$, $\cos x$, $(1-k^2\sin^2 x)^{\frac{1}{2}}$, if we also replace dx by $\dfrac{dx}{(1-k^2\sin^2 x)^{\frac{1}{2}}}$, and make the

necessary changes in the limits. This is evident, since

$$\frac{d\,\mathrm{am}\,x}{dx}=\mathrm{dn}\,x,$$

and as $\mathrm{am}\,\mathrm{K}=\frac{1}{2}\pi$, $\mathrm{am}\,2\mathrm{K}=\pi$, we see that to the limits K or 2K and correspond respectively the limits $\frac{1}{2}\pi$ or π and 0.

Thus (5), (6), (7) may be written

$$\int_0^{\frac{1}{2}\pi}\frac{\log\sin x}{(1-k^2\sin^2 x)^{\frac{1}{2}}}\,dx=-\tfrac{1}{4}\pi\mathrm{K}'-\tfrac{1}{2}\mathrm{K}\log k,$$

$$\int_0^{\frac{1}{2}\pi}\frac{\log\cos x}{(1-k^2\sin^2 x)^{\frac{1}{2}}}\,dx=-\tfrac{1}{4}\pi\mathrm{K}'+\tfrac{1}{2}\mathrm{K}\log k,$$

$$\int_0^{\frac{1}{2}\pi}\frac{\log(1-k^2\sin^2 x)}{(1-k^2\sin^2 x)^{\frac{1}{2}}}\,dx=\mathrm{K}\log k',$$

while, for example, (57) becomes

$$\int_0^{\frac{1}{2}\pi}\frac{\log\{1-(1-k')\sin^2 x\}}{(1-k^2\sin^2 x)^{\frac{1}{2}}}\,dx=\tfrac{1}{2}\mathrm{K}\log\left(\frac{2k'^{\frac{1}{2}}}{1+k'}\right),$$

and the other integrals may be similarly transformed.

In this form several of the above integrals have been obtained by Mr. William Roberts in his papers "Sur l'Evaluation de quelques Intégrales Définies par les Fonctions Elliptiques" ("Liouville's Journal," t. xi, 1846, pp. 157—173), "Sur l'Intégrale Définie $\int_0^{\frac{1}{2}\pi}\frac{\log(1+n\sin^2\phi)}{(1-k^2\sin^2\phi)^{\frac{1}{2}}}d\phi$," (Id., pp. 471—476), and "Note sur quelques Intégrales Transcendantes" (Id., t. xii, 1847, pp. 449—456), which contain evaluations equivalent to (5), (6), (7), (16), (38), (44), (48), (57), (59), (60). In the first of these papers the value of the integral in (7) is found by means of the transformation $k'\tan\phi\tan\psi=1$, which is equivalent to the substitution of $\mathrm{K}-x$ for x; and in the last paper the values of the integrals in (5) and (6) are found directly from the q-products, viz.:

$$\mathrm{sn}\left(\frac{2\mathrm{K}x}{\pi}\right)=\frac{2q^{\frac{1}{4}}}{k^{\frac{1}{2}}}\sin x\,\frac{(1-2q^2\cos 2x+q^4)(1-2q^4\cos 2x+q^8)\ldots}{(1-2q\cos 2x+q^2)(1-2q^3\cos 2x+q^6)\ldots}$$

$$\mathrm{cn}\left(\frac{2\mathrm{K}x}{\pi}\right)=2\left(\frac{k'}{k}\right)^{\frac{1}{2}}q^{\frac{1}{4}}\cos x\,\frac{(1+2q^2\cos 2x+q^4)(1+2q^4\cos 2x+q^8)\ldots}{(1-2q\cos 2x+q^2)(1-2q^3\cos 2x+q^6)\ldots}$$

by means of the integral (1), which gives

$$\int_0^{\frac{1}{2}\pi}\log(1-2q^n\cos 2x+q^{2n})\,dx=0.$$

The equation (38) is also obtained directly from the q-product for Θ; this is of course practically equivalent to the use of the cosine series (52) for $\log\Theta x$ quoted in § 11.

The last two papers relate chiefly to the integral

$$\int_0^{\mathrm{K}} \log(1+n\,\mathrm{sn}^2 x)dx,$$

and one of the methods indicated for obtaining its value depends upon the formula

$$2\Upsilon\,\mathrm{am}\,p+2\Upsilon\,\mathrm{am}\,q=\Upsilon\,\mathrm{am}\,(p+q)+\Upsilon\,\mathrm{am}\,(p-q)-\log(1-k^2\,\mathrm{sn}^2 p\,\mathrm{sn}^2 q),$$

and is therefore the same in principle as that employed in § 10. Mr. Roberts generally uses the Legendrian notation and the function Υ, for example the formula (44), viz.:

$$\int_0^{\mathrm{K}} \log(1-k^2\,\mathrm{sn}^2 x\,\mathrm{sn}^2 y)dx=2\mathrm{K}\log\frac{\Theta 0}{\Theta y}$$

is written

$$\int_0^{\frac{1}{2}\pi}\frac{\log(1-k^2\sin^2\theta\sin^2\phi)}{(1-k^2\sin^2\phi)^{\frac{1}{2}}}d\phi=\mathrm{E}(k)[\mathrm{F}(k,\theta)]^2-2\mathrm{F}(k)\Upsilon(k,\theta),$$

but he remarks ("Liouville," t. xii, p. 453) that some of the results could have been obtained more readily by the use of Θ instead of Υ.

Mr. Roberts also gives the values of the integrals

$$\int_0^{\frac{1}{2}\pi}\frac{\log(1+\cot^2\theta\sin^2\phi)}{(1-k^2\sin^2\phi)^{\frac{1}{2}}}d\phi,$$

$$\int_0^{\frac{1}{2}\pi}\frac{\log\{1-(1-k'^2\sin^2\theta)\sin^2\phi\}}{(1-k^2\sin^2\phi)^{\frac{1}{2}}}d\phi,$$

$$\int_0^{\frac{1}{2}\pi}\int_0^{\frac{1}{2}\pi}\frac{\log(1+\cot^2\theta\sin^2\phi)}{(1-k^2\sin^2\phi)^{\frac{1}{2}}(1-k'^2\sin^2\theta)^{\frac{1}{2}}}d\theta d\phi,$$

$$\int_0^{\frac{1}{2}\pi}\int_0^{\frac{1}{2}\pi}\frac{\log\{1-(1-k'^2\sin^2\theta)\sin^2\phi\}}{(1-k^2\sin^2\phi)^{\frac{1}{2}}(1-k'^2\sin^2\theta)^{\frac{1}{2}}}d\theta d\phi.$$

These can be derived from (44) by the substitution of $i\mathrm{K}'-iy$ and $\mathrm{K}+i\mathrm{K}'-iy$ in place of y, for

$$k\,\mathrm{sn}(i\mathrm{K}'-iy)=i\frac{\mathrm{cn}\,(y,k')}{\mathrm{sn}\,(y,k')},$$

$$k\,\mathrm{sn}\,(\mathrm{K}+i\mathrm{K}'-iy)=\mathrm{dn}\,(y,k'),$$

$$\Theta(i\mathrm{K}'-iy)=\frac{k'^{\frac{1}{2}}\mathrm{K}^{\frac{1}{2}}}{\mathrm{K}'^{\frac{1}{2}}}e^{\frac{\pi}{4\mathrm{K}\mathrm{K}'}(\mathrm{K}'-y)^2}\,\mathrm{sn}\,(y,k')\Theta(y,k'),$$

$$\Theta(\mathrm{K}+i\mathrm{K}'-iy)=\frac{1}{k^{\frac{1}{2}}}\frac{1}{\mathrm{sn}\,(y,k')}\Theta(i\mathrm{K}'-iy),$$

whence we find

$$\int_0^{\mathrm{K}}\log\left\{1+\frac{\mathrm{cn}^2\,(y,k')}{\mathrm{sn}^2\,(y,k')}\mathrm{sn}^2 x\right\}dx$$

$$=-\tfrac{1}{2}\frac{\pi}{\mathrm{K}'}(\mathrm{K}'-y)^2+\mathrm{K}\log\left(\frac{2\mathrm{K}'}{\pi}\right)-2\mathrm{K}\log\Theta(y,k')-2\mathrm{K}\log\mathrm{sn}(y,k')\ (63),$$

$$\int_0^{\mathrm{K}} \log\{1-\mathrm{dn}^2(y,k')\,\mathrm{sn}^2 x\}dx$$

$$=-\tfrac{1}{2}\frac{\pi}{\mathrm{K}'}(\mathrm{K}'-y)^2+\mathrm{K}\log\left(\frac{2k'\mathrm{K}'}{\pi}\right)-2\mathrm{K}\log\Theta(y,k') \quad . \quad . \quad . \quad (64),$$

and integrating with regard to y between the limits K′ and 0 these give

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}'} \log\left\{1+\frac{\mathrm{cn}^2(y,k')}{\mathrm{sn}^2(y,k')}\mathrm{sn}^2 x\right\}dx=\tfrac{1}{3}\pi\mathrm{K}^2-\tfrac{1}{6}\pi\mathrm{K}'^2+\tfrac{1}{3}\mathrm{K}\mathrm{K}'\log\left(\frac{4k'^2}{k}\right). \quad (65),$$

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}'} \log\{1-\mathrm{dn}^2(y,k')\,\mathrm{sn}^2 x\}dx=-\tfrac{1}{6}\pi\mathrm{K}^2-\tfrac{1}{6}\pi\mathrm{K}'^2+\tfrac{1}{3}\mathrm{K}\mathrm{K}'\log\left(\frac{4k'^2}{k}\right) (66),$$

which are in fact Mr. Roberts's results ("Liouville," t. xii, p. 456). In the "Philosophical Magazine," for December, 1860,* Professor Sylvester has given the values of two integrals which are equivalent to (5) and (28). The method depends upon an expansion in a series and is quite different to any employed in this paper. Professor Sylvester notices particularly the fact that the values of the integrals in (5) and (28) should be equal in magnitude.

§ 14. Recently in the "Mathematische Annalen" of Clebsch and Neumann (t. xi, 1877, pp. 567—570), Dr. Enneper has obtained the evaluation

$$\int_0^{\mathrm{K}} \mathrm{dn}^2 x \log \mathrm{dn}\, x dx=\tfrac{1}{2}\mathrm{E}\log k'+\tfrac{1}{2}(1+k'^2)\mathrm{K}-\mathrm{E}$$

by the substitution of $\mathrm{K}-x$ for x and further transformations: but the value of this integral had been previously found in a somewhat similar manner by Mr. Roberts, in a note in "Liouville's Journal" for 1846 ("Extrait d'une Lettre addressée à M. Liouville," t. xi, pp. 343, 344).

Dr. Enneper, in his paper, by the use of (33) shows that

$$\int_0^{\mathrm{K}} \arctan\left\{\frac{k'}{\mathrm{dn}\,(x+a)\,\mathrm{dn}\,(x-a)}\right\}dx$$

$$=\int_0^{\mathrm{K}} \arctan\left\{\frac{\mathrm{dn}\,(x+a)\,\mathrm{dn}\,(x-a)}{k'}\right\}dx=\tfrac{1}{4}\pi\mathrm{K} \quad . \quad . \quad (67),$$

and it may be observed that, by following the same method, we have at once

* "Notes to the Meditation on Poncelet's Theorem, including a Valuation of the two new Definite Integrals

$$\int_0^{\frac{\pi}{2}}\frac{\log\cos\phi}{\sqrt{1-b^2(\cos\phi)^2}}d\phi, \qquad \int_0^{\frac{\pi}{2}}\frac{\log\{1+\sqrt{1-b^2(\cos\phi)^2}\}}{\sqrt{1-b^2(\cos\phi)^2}}d\phi,"$$

"Phil. Mag.," ser. iv, t. xx, pp. 525—533 (1860). See also, by the same author, "Note on certain Definite Integrals," "Quarterly Journal of Mathematics," t. iv, pp. 319—324 (1861).

$$\int_0^{\mathrm{K}} \operatorname{arc\,tan}\left(\frac{\operatorname{dn} x}{k'^{\frac{1}{2}}}\right)dx = \int_0^{\mathrm{K}} \operatorname{arc\,tan}\left(\frac{k'^{\frac{1}{2}}}{\operatorname{dn} x}\right)dx$$

$$= \tfrac{1}{2}\int_0^{\mathrm{K}} \left\{ \operatorname{arc\,tan}\left(\frac{\operatorname{dn} x}{k'^{\frac{1}{2}}}\right) + \operatorname{arc\,tan}\left(\frac{k'^{\frac{1}{2}}}{\operatorname{dn} x}\right)\right\} dx$$

$$= \tfrac{1}{2}\int_0^{\mathrm{K}} \tfrac{1}{2}\pi dx = \tfrac{1}{4}\pi \mathrm{K} \quad \ldots\ldots\ldots\ldots \quad (68).$$

Also, n being any quantity, we see from (33) that

$$\int_0^{\mathrm{K}} \frac{dx}{1+\dfrac{\operatorname{dn}^n x}{k'^{\frac{1}{2}n}}} = \int_0^{\mathrm{K}} \frac{dx}{1+\dfrac{k'^{\frac{1}{2}n}}{\operatorname{dn}^n x}}$$

$$= \tfrac{1}{2}\int_0^{\mathrm{K}} \left(\frac{k'^{\frac{1}{2}n}}{k'^{\frac{1}{2}n}+\operatorname{dn}^n x} + \frac{\operatorname{dn}^n x}{k'^{\frac{1}{2}n}+\operatorname{dn}^n x}\right)dx$$

$$= \tfrac{1}{2}\mathrm{K}.$$

so that

$$\int_0^{\mathrm{K}} \frac{dx}{k'^{\frac{1}{2}n}+\operatorname{dn}^n x} = \tfrac{1}{2}\frac{\mathrm{K}}{k'^{\frac{1}{2}n}} \quad \ldots\ldots\ldots \quad (69),$$

$$\int_0^{\mathrm{K}} \frac{\operatorname{dn}^n x}{k'^{\frac{1}{2}n}+\operatorname{dn}^n x}dx = \tfrac{1}{2}\mathrm{K} \quad \ldots\ldots\ldots \quad (70),$$

and, similarly,

$$\int_0^{\mathrm{K}} \frac{k'^n}{k'^n+\operatorname{dn}^n(x-a)\operatorname{dn}^n(x+a)}dx$$

$$= \int_0^{\mathrm{K}} \frac{\operatorname{dn}^n(x-a)\operatorname{dn}^n(x+a)}{k'^n+\operatorname{dn}^n(x-a)\operatorname{dn}^n(x+a)}dx$$

$$= \tfrac{1}{2}\mathrm{K} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (71).$$

Further, if ϕ be an uneven function and n any quantity,

$$\int_0^{\mathrm{K}} \phi\left(\frac{\operatorname{dn} x}{k'^{\frac{1}{2}}} - \frac{k'^{\frac{1}{2}}}{\operatorname{dn} x}\right)\log\left(1+\frac{\operatorname{dn}^n x}{k'^{\frac{1}{2}n}}\right)dx$$

$$= -\int_0^{\mathrm{K}} \phi\left(\frac{\operatorname{dn} x}{k'^{\frac{1}{2}}} - \frac{k'^{\frac{1}{2}}}{\operatorname{dn} x}\right)\log\left(1+\frac{k'^{\frac{1}{2}n}}{\operatorname{dn}^n x}\right)dx,$$

whence

$$\int_0^{\mathrm{K}} \phi\left(\frac{\operatorname{dn} x}{k'^{\frac{1}{2}}} - \frac{k'^{\frac{1}{2}}}{\operatorname{dn} x}\right)\log\left(k'^{\frac{1}{2}n}+\operatorname{dn}^n x\right)dx$$

$$= \tfrac{1}{4}n\log k'\int_0^{\mathrm{K}} \phi\left(\frac{\operatorname{dn} x}{k'^{\frac{1}{2}}} - \frac{k'^{\frac{1}{2}}}{\operatorname{dn} x}\right)dx + \tfrac{1}{2}n\int_0^{\mathrm{K}} \phi\left(\frac{\operatorname{dn} x}{k'^{\frac{1}{2}}} - \frac{k'^{\frac{1}{2}}}{\operatorname{dn} x}\right)\log \operatorname{dn} x\, dx,$$

and of course there are other formulæ involving $\dfrac{\operatorname{dn} x}{k'^{\frac{1}{2}}}$ and $\dfrac{\operatorname{dn}(x-a)\operatorname{dn}(x+a)}{k'}$ which may be proved in a similar manner.

§ 15. It was shown in § 5 that

$$\int_0^{\mathrm{K}}\int_{-\mathrm{K}'}^{\mathrm{K}'} \log \mathrm{cn}\,(x+iy)\,dxdy = \mathrm{K}\mathrm{K}' \log \frac{k'}{k};$$

the value of this integral when the limits with regard to y are K′ and 0 may be found as follows.

In the formula

$$\mathrm{cn}\,u = \frac{k'^{\frac{1}{4}}}{k^{\frac{1}{4}}} q^{\frac{1}{4}} e^{\frac{\pi i u}{2\mathrm{K}}} \frac{\Theta(u+\mathrm{K}+i\mathrm{K}')}{\Theta u},$$

put

$$u = x+iy-\mathrm{K}-i\mathrm{K}',$$

and, taking logarithms, we find

$$\log \mathrm{cn}\,(x+iy-\mathrm{K}-i\mathrm{K}') = \tfrac{1}{2}\log\frac{k'}{k} - \tfrac{1}{4}\frac{\pi \mathrm{K}'}{\mathrm{K}} + \tfrac{1}{2}\frac{\pi i}{\mathrm{K}}(x+iy-\mathrm{K}-i\mathrm{K}')$$

$$+\log\Theta(x+iy) - \log\Theta(x+iy-\mathrm{K}-i\mathrm{K}') \quad . \quad . \quad . \quad (72)$$

Now

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}'} \phi(x+iy-\mathrm{K}-i\mathrm{K}')\,dxdy = \int_0^{\mathrm{K}}\int_0^{\mathrm{K}'} \phi(x+iy)\,dxdy$$

if ϕ be an even function, so that (72) gives

$$\int_0^{\mathrm{K}}\int_0^{\mathrm{K}'} \log \mathrm{cn}\,(x+iy)\,dxdy = \tfrac{1}{2}\mathrm{K}\mathrm{K}'\log\frac{k'}{k} - \tfrac{1}{4}\pi \mathrm{K}'^2$$

$$+\tfrac{1}{2}\frac{\pi i}{\mathrm{K}}(\tfrac{1}{2}\mathrm{K}^2\mathrm{K}' + \tfrac{1}{2}i\mathrm{K}\mathrm{K}'^2 - \mathrm{K}^2\mathrm{K}' - i\mathrm{K}\mathrm{K}'^2)$$

$$= \tfrac{1}{2}\mathrm{K}\mathrm{K}'\log\frac{k'}{k} - \tfrac{1}{4}i\pi\mathrm{K}\mathrm{K}' \quad . \quad . \quad . \quad . \quad . \quad (73).$$

§ 16. I conclude with the determination of the values of the integrals

$$\int_0^\infty e^{-x^2}\Theta\left\{2x\left(\frac{n\mathrm{K}\mathrm{K}'}{\pi}\right)^{\frac{1}{2}}\right\}dx, \qquad \int_0^\infty x e^{-x^2}\mathrm{H}\left\{2x\left(\frac{n\mathrm{K}\mathrm{K}'}{\pi}\right)^{\frac{1}{2}}\right\}dx$$

when n is a positive integer.

$$\Theta\left(\frac{2\mathrm{K}x}{\pi}\right) = 1 - 2q\cos 2x + 2q^4\cos 4x - 2q^9\cos 6x + \text{ \&c.},$$

whence

$$e^{-x^2}\Theta\left(\frac{2\mathrm{K}ax}{\pi}\right) = e^{-x^2} - 2qe^{-x^2}\cos 2ax + 2q^4e^{-x^2}\cos 4ax - \text{ \&c.}$$

Now

$$\int_0^\infty e^{-x^2}\cos 2bx\,dx = \tfrac{1}{2}\pi^{\frac{1}{2}}e^{-b^2},$$

so that we have

$$\int_0^\infty e^{-x^2}\Theta\left(\frac{2\mathrm{K}ax}{\pi}\right)dx = \tfrac{1}{2}\pi^{\frac{1}{2}}(1 - 2qe^{-a^2} + 2q^4e^{-4a^2} - 2q^9e^{-9a^2} + \text{ \&c.})$$

Let $$e^{-a^2}=q^n,$$

n being a positive integer, whence

$$a=\left(\frac{n\pi \mathrm{K}'}{\mathrm{K}}\right)^{\frac{1}{2}},$$

and

$$\int_0^\infty e^{-x^2}\Theta\left\{2x\left(\frac{n\mathrm{K}\mathrm{K}'}{\pi}\right)^{\frac{1}{2}}\right\}dx=\tfrac{1}{2}\pi^{\frac{1}{2}}(1-2q^{n+1}-2q^{4(n+1)}+2q^{9(n+1)}-\text{ \&c.})$$

$$=\tfrac{1}{2}\pi^{\frac{1}{2}}\left(\frac{2k'_{n+1}\mathrm{K}_{n+1}}{\pi}\right)^{\frac{1}{2}}$$

$$=\left(\frac{k'_{n+1}\mathrm{K}_{n+1}}{2}\right)^{\frac{1}{2}} \quad \ldots\ldots\ldots \quad (74),$$

where k'_{n+1}, K_{n+1} are what k and K become when q is replaced by q^{n+1}, so that k_{n+1} is the modulus obtained by the first real transformation of the $(n+1)^{\text{th}}$ order.

As a particular case put $n=1$, and the formula (74) gives

$$\int_0^\infty e^{-x^2}\Theta\left\{2x\left(\frac{\mathrm{K}\mathrm{K}'}{\pi}\right)^{\frac{1}{2}}\right\}dx=(\tfrac{1}{2}\mathrm{K}k'^{\frac{1}{2}})^{\frac{1}{2}} \quad \ldots\ldots \quad (75).$$

To evaluate the other integral, multiply the equation

$$\mathrm{H}\left(\frac{2\mathrm{K}ax}{\pi}\right)=2q^{\frac{1}{4}}\sin ax-2q^{\frac{9}{4}}\sin 3ax+2q^{\frac{25}{4}}\sin 5ax-\text{ \&c.}$$

by e^{-x^2} and integrate, replacing the integrals on the right hand side by their values from the formula

$$\int_0^\infty xe^{-x^2}\sin 2bx\,dx=\tfrac{1}{2}\pi^{\frac{1}{2}}be^{-b^2},$$

we thus find

$$\int_0^\infty xe^{-x^2}\mathrm{H}\left(\frac{2\mathrm{K}ax}{\pi}\right)dx=\tfrac{1}{2}\pi^{\frac{1}{2}}a(q^{\frac{1}{4}}e^{-\frac{1}{4}a^2}-3q^{\frac{9}{4}}e^{-\frac{9}{4}a^2}+5q^{\frac{25}{4}}e^{-\frac{25}{4}a^2}-\text{ \&c.})$$

Put, as before, $$e^{-a^2}=q^n,$$

n being a positive integer, then

$$a=\left(\frac{n\pi \mathrm{K}'}{\mathrm{K}}\right)^{\frac{1}{2}},$$

and the equation becomes

$$\int_0^\infty xe^{-x^2}\mathrm{H}\left\{2x\left(\frac{n\mathrm{K}\mathrm{K}'}{\pi}\right)^{\frac{1}{2}}\right\}dx=\tfrac{1}{2}\pi\left(\frac{n\mathrm{K}'}{\mathrm{K}}\right)^{\frac{1}{2}}(q^{\frac{1}{4}(n+1)}-3q^{\frac{9}{4}(n+1)}+5q^{\frac{25}{4}(n+1)}-\text{ \&c.})$$

But $$2q^{\frac{1}{4}}-6q^{\frac{9}{4}}+10q^{\frac{25}{4}}-\text{ \&c.}=\left\{kk'\left(\frac{2\mathrm{K}}{\pi}\right)^3\right\}$$

("Fundamenta Nova," p. 184), so that

$$\int_0^\infty xe^{-x^2}\mathrm{H}\left\{2x\left(\frac{n\mathrm{K}\mathrm{K}'}{\pi}\right)^{\frac{1}{2}}\right\}dx=\left\{\tfrac{1}{2}\frac{n\mathrm{K}'}{\pi\mathrm{K}}k_{n+1}k'_{n+1}\mathrm{K}^3{}_{n+1}\right\}^{\frac{1}{2}} \quad . \quad (76).$$

Put $n=1$ and this formula gives

$$\int_0^\infty xe^{-x^2}\mathrm{H}\left\{2x\left(\frac{\mathrm{K}\mathrm{K}'}{\pi}\right)^{\frac{1}{2}}\right\}dx=\tfrac{1}{2}\mathrm{K}k\left(\tfrac{1}{2}\frac{\mathrm{K}'k'^{\frac{1}{2}}}{\pi}\right)^{\frac{1}{2}} \quad . \quad . \quad (77).$$

IX. "Values of the Theta and Zeta Functions for certain Values of the Argument." By J. W. L. Glaisher, M.A., F.R.S., Fellow of Trinity College, Cambridge. Received July 31, 1879.

§ 1. In § 12 of the preceding paper "On Definite Integrals involving Elliptic Functions," it was necessary to determine the values of $\Theta(\frac{1}{2}\mathrm{K})$, $\Theta(\frac{1}{2}\mathrm{K}+i\mathrm{K}')$, $\Theta(\frac{1}{2}i\mathrm{K}')$, $\Theta(\mathrm{K}+\frac{1}{2}i\mathrm{K}')$, and $\Theta^4(\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}')$, which were required in the evaluation of some of the integrals. This led me to calculate a table of the values of the Θ and H functions when the arguments were of the form $\mathrm{K}+ni\mathrm{K}'$, for the values 0, $\frac{1}{2}$, 1, $\frac{3}{2}$, of m and n, and the results are contained in this paper. For the sake of completeness the corresponding values of the Z function are also given; and some remarks connected with the q-series to which the formulæ lead are added.

A table of the values of the sn, cn, dn for the above-mentioned arguments is given by Professor Cayley on page 74 of his "Elementary Treatise on Elliptic Functions" (1876): this table* is so useful that it seemed desirable to supplement it by a similar one for the Θ, H, and Z functions.

§ 2. The values found in § 13 are

$$\Theta(\tfrac{1}{2}\mathrm{K}) \quad =\frac{2^{\frac{1}{2}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k'^{\frac{1}{4}}(1+k')^{\frac{1}{2}},$$

$$\Theta(\tfrac{1}{2}\mathrm{K}+i\mathrm{K}') \quad =q^{-\frac{1}{8}}\frac{\mathrm{K}^{\frac{1}{2}}}{2^{\frac{1}{2}}\pi^{\frac{1}{4}}}k'^{\frac{1}{4}}(1-k')^{\frac{1}{2}}(1+i),$$

$$\Theta(\tfrac{1}{2}i\mathrm{K}') \quad =q^{-\frac{1}{16}}\frac{2^{\frac{1}{2}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{4}}}k^{\frac{1}{4}}(1-k)^{\frac{1}{2}},$$

* I may here note that the value of $\mathrm{cn}(\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}')$ should be $\frac{1-i}{\sqrt{2}}\frac{\sqrt{k'}}{\sqrt{k}}$ instead of $-\frac{1-i}{\sqrt{2}}\frac{\sqrt{k'}}{\sqrt{k}}$.

$$\Theta(\mathrm{K}+\tfrac{1}{2}i\mathrm{K}') = q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{4}}(1+k)^{\frac{1}{4}},$$

$$\Theta^4(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}') = q^{-\frac{1}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(k'+ik).$$

From this last equation the value of $\Theta(\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}')$ may be deduced; but it is more convenient to avoid ambiguities of sign as follows.

In the formula

$$\Theta(u+v)\Theta(u-v) = \frac{\Theta^2 u\Theta^2 v}{\Theta^2 0}(1-k^2\,\mathrm{sn}^2 u\,\mathrm{sn}^2 v),$$

put $u=\frac{1}{2}\mathrm{K}$, $v=\frac{1}{2}i\mathrm{K}'$, then

$$\begin{aligned}\Theta(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')\Theta(\tfrac{1}{2}\mathrm{K}-\tfrac{1}{2}i\mathrm{K}') &= \frac{\pi}{2k'\mathrm{K}}q^{-\frac{1}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(1+k')^{\frac{1}{2}}(1-k)^{\frac{1}{2}}\left\{1-k^2\frac{1}{1+k'}\frac{i^2}{k}\right\}\\ &= q^{-\frac{1}{4}}\frac{\mathrm{K}}{k'\pi}k^{\frac{1}{2}}k'^{\frac{1}{2}}\left(\frac{1-k}{1+k'}\right)^{\frac{1}{2}}(1+k+k')\\ &= q^{-\frac{1}{4}}\frac{2^{\frac{1}{2}}\mathrm{K}}{\pi}k^{\frac{1}{2}}k'^{\frac{1}{2}} \quad \ldots\ldots\ldots\

where θ is the modular angle, it follows that

$$\Theta^2(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')=q^{-\frac{1}{8}}\frac{2^{\frac{1}{2}}\mathrm{K}}{\pi}k^{\frac{1}{2}}k'^{\frac{1}{2}}(\cos\tfrac{1}{2}\theta+i\sin\tfrac{1}{2}\theta),$$

and
$$\Theta(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')=q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{4}}k'^{\frac{1}{4}}(\cos\tfrac{1}{4}\theta+i\sin\tfrac{1}{4}\theta).$$

This last result is readily identified with that found above, since

$$\cos\tfrac{1}{4}\theta=\frac{\{2^{\frac{1}{2}}+(1+k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{3}{4}}},\qquad \sin\tfrac{1}{4}\theta=\frac{\{2^{\frac{1}{2}}-(1+k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{3}{4}}}.$$

§ 3. From the values of Θu for $u=\frac{1}{2}\mathrm{K}$, $\frac{1}{2}i\mathrm{K}'$, $\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$, the values of Θu for the other arguments contained in the table were deduced by means of the formulæ

$$\Theta(u+\mathrm{K})=k'^{-\frac{1}{2}}\mathrm{dn}\,u\,\Theta u,$$

$$\Theta(u+i\mathrm{K}')=iq^{-\frac{1}{4}}k^{\frac{1}{2}}e^{-\frac{i\pi u}{2\mathrm{K}}}\,\mathrm{sn}\,u\,\Theta u.$$

The values of $\mathrm{H}u$ were deduced from those of Θu by the formula

$$\mathrm{H}u=-iq^{\frac{1}{4}}e^{\frac{i\pi u}{2\mathrm{K}}}\Theta(u+i\mathrm{K}').$$

For example,

$$\begin{aligned}\mathrm{H}(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')&=-iq^{\frac{1}{4}}e^{\frac{i\pi}{2\mathrm{K}}(\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}')}\Theta(\tfrac{1}{2}\mathrm{K}+\tfrac{3}{2}i\mathrm{K}')\\&=-iq^{\frac{1}{4}}e^{\frac{1}{4}i\pi}q^{\frac{1}{4}}\,.\,q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{4}}k'^{\frac{1}{4}}(\sin\tfrac{1}{4}\theta+i\cos\tfrac{1}{4}\theta)\\&=q^{-\frac{1}{16}}\frac{\mathrm{K}^{\frac{1}{2}}}{2^{\frac{1}{4}}\pi^{\frac{1}{2}}}k^{\frac{1}{4}}k'^{\frac{1}{4}}(1-i)(\sin\tfrac{1}{4}\theta+i\cos\tfrac{1}{4}\theta)\\&=q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{\cos\tfrac{1}{4}(\pi-\theta)+i\sin\tfrac{1}{4}(\pi-\theta)\},\end{aligned}$$

and it can be shown that

$$\cos\tfrac{1}{4}(\pi-\theta)=\frac{\{2^{\frac{1}{2}}+(1-k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{3}{4}}},\qquad \sin\tfrac{1}{4}(\pi-\theta)=\frac{\{2^{\frac{1}{2}}-(1-k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{3}{4}}}.$$

The values in the table were verified throughout by the formulæ

$$\frac{\mathrm{H}u}{\Theta u}=k^{\frac{1}{2}}\,\mathrm{sn}\,u,\qquad \frac{\mathrm{H}(u+\mathrm{K})}{\Theta u}=\frac{k^{\frac{1}{2}}}{k'^{\frac{1}{2}}}\,\mathrm{cn}\,u;$$

several of the results are of course deducible at once from one another by the relations $\Theta(u+2\mathrm{K})=\Theta u$, $\mathrm{H}(u+2\mathrm{K})=-\mathrm{H}u$, and a change in the sign of i.

Values of Θu and $\mathrm{H}u$.

u.	Θu.	$\mathrm{H}u$.
$\frac{1}{2}\mathrm{K}$	$\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1+k')^{\frac{1}{4}}$	$\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1-k')^{\frac{1}{4}}$
$\frac{3}{2}\mathrm{K}$	$\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1+k')^{\frac{1}{4}}$	$\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1-k')^{\frac{1}{4}}$
$\frac{1}{2}\mathrm{K}+i\mathrm{K}'$	$q^{-\frac{1}{4}}\frac{\mathrm{K}^{\frac{1}{2}}}{2^{\frac{1}{4}}\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1-k')^{\frac{1}{4}}(1+i)$	$q^{-\frac{1}{4}}\frac{\mathrm{K}^{\frac{1}{2}}}{2^{\frac{1}{4}}\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1+k')^{\frac{1}{4}}(1+i)$
$\frac{3}{2}\mathrm{K}+i\mathrm{K}'$	$q^{-\frac{1}{4}}\frac{\mathrm{K}^{\frac{1}{2}}}{2^{\frac{1}{4}}\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1-k')^{\frac{1}{4}}(1-i)$	$q^{-\frac{1}{4}}\frac{\mathrm{K}^{\frac{1}{2}}}{2^{\frac{1}{4}}\pi^{\frac{1}{2}}}k'^{\frac{1}{6}}(1+k')^{\frac{1}{4}}(1-i)$
$\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1-k)^{\frac{1}{4}}$	$iq^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1-k)^{\frac{1}{4}}$
$\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1+k)^{\frac{1}{4}}$	$q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1+k)^{\frac{1}{4}}$
$\frac{3}{2}i\mathrm{K}'$	$-q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1-k)^{\frac{1}{4}}$	$iq^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1-k)^{\frac{1}{4}}$
$\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1+k)^{\frac{1}{4}}$	$q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}(1+k)^{\frac{1}{4}}$
$\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\cos\frac{1}{4}\theta+i\sin\frac{1}{4}\theta\}$	$q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\cos\frac{1}{4}(\pi-\theta)+i\sin\frac{1}{4}(\pi-\theta)$
$\frac{3}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\cos\frac{1}{4}\theta-i\sin\frac{1}{4}\theta\}$	$q^{-\frac{1}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\cos\frac{1}{4}(\pi-\theta)-i\sin\frac{1}{4}(\pi-\theta)$
$\frac{1}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\sin\frac{1}{4}\theta+i\cos\frac{1}{4}\theta\}$	$q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\sin\frac{1}{4}(\pi-\theta)+i\cos\frac{1}{4}(\pi-\theta)$
$\frac{3}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\sin\frac{1}{4}\theta-i\cos\frac{1}{4}\theta\}$	$q^{-\frac{9}{16}}\frac{2^{\frac{1}{4}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}\{\sin\frac{1}{4}(\pi-\theta)-i\cos\frac{1}{4}(\pi-\theta)$

where θ is the modular angle, and

$$\cos\tfrac{1}{4}\theta=\frac{\{2^{\frac{1}{2}}+(1+k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{1}{2}}},\qquad \sin\tfrac{1}{4}\theta=\frac{\{2^{\frac{1}{2}}-(1+k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{1}{2}}},$$

$$\cos\tfrac{1}{4}(\pi-\theta)=\frac{\{2^{\frac{1}{2}}+(1-k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{1}{2}}},\qquad \sin\tfrac{1}{4}(\pi-\theta)=\frac{\{2^{\frac{1}{2}}-(1-k')^{\frac{1}{2}}\}^{\frac{1}{2}}}{2^{\frac{1}{2}}},$$

§ 4. Also, squaring the values in the last group,

$$\Theta^2(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')=q^{-\frac{1}{8}}\frac{2^{\frac{1}{2}}\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{\cos\tfrac{1}{2}\theta+i\sin\tfrac{1}{2}\theta\},\ \&c.,$$

so that we have the following table:—

u.	$\Theta^2 u$.	$\mathrm{H}^2 u$.
$\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1+k')^{\frac{1}{2}}+i(1-k')^{\frac{1}{2}}\}$	$q^{-\frac{1}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1-k')^{\frac{1}{2}}+i(1+k')^{\frac{1}{2}}\}$
$\frac{3}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1+k')^{\frac{1}{2}}-i(1-k')^{\frac{1}{2}}\}$	$q^{-\frac{1}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1-k')^{\frac{1}{2}}-i(1+k')^{\frac{1}{2}}\}$
$\frac{1}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$-q^{-\frac{9}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1+k')^{\frac{1}{2}}-i(1-k')^{\frac{1}{2}}\}$	$-q^{-\frac{9}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1-k')^{\frac{1}{2}}-i(1+k')^{\frac{1}{2}}\}$
$\frac{3}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$-q^{-\frac{9}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1+k')^{\frac{1}{2}}+i(1-k')^{\frac{1}{2}}\}$	$-q^{-\frac{9}{8}}\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}}\{(1-k')^{\frac{1}{2}}+i(1+k')^{\frac{1}{2}}\}$

and, squaring again,

u.	$\Theta^4 u$.	$\mathrm{H}^4 u$.
$\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(k'+ik)$	$q^{-\frac{1}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(-k'+ik)$
$\frac{3}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$q^{-\frac{1}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(k'-ik)$	$q^{-\frac{1}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(-k'-ik)$
$\frac{1}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$q^{-\frac{9}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(k'-ik)$	$q^{-\frac{9}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(-k'-ik)$
$\frac{3}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$q^{-\frac{9}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(k'+ik)$	$q^{-\frac{9}{4}}\frac{2\mathrm{K}^2}{\pi^2}k^{\frac{1}{2}}k'^{\frac{1}{2}}(-k'+ik)$

§ 5. In the case of the Z function, we have

$$\mathrm{Z}(\tfrac{1}{2}\mathrm{K}) \quad =\tfrac{1}{2}(1-k'),$$

$$\mathrm{Z}(\tfrac{1}{2}i\mathrm{K}') \quad =-\tfrac{1}{4}\frac{i\pi}{\mathrm{K}}+\tfrac{1}{2}i(1+k),$$

$$\mathrm{Z}(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')=-\tfrac{1}{4}\frac{i\pi}{\mathrm{K}}+\tfrac{1}{2}(k+ik'),$$

and the following formulæ

$$\mathrm{Z}(u+v) = \mathrm{Z}u+\mathrm{Z}v-k^2\,\mathrm{sn}\,u\,\mathrm{sn}\,v\,\mathrm{sn}\,(u+v),$$

$$\mathrm{Z}(u+i\mathrm{K}') = -\tfrac{1}{2}\frac{i\pi}{\mathrm{K}}+\mathrm{Z}u+\frac{\mathrm{cn}\,u\,\mathrm{dn}\,u}{\mathrm{sn}\,u},$$

$$\mathrm{Z}(u+2i\mathrm{K}') = -\frac{i\pi}{\mathrm{K}}+\mathrm{Z}u.$$

From these we obtain the following table:—

Values of $\mathrm{Z}u$.

u.	$\mathrm{Z}u$.
$\frac{1}{2}\mathrm{K}$	$\frac{1}{2}(1-k')$
$\frac{3}{2}\mathrm{K}$	$-\frac{1}{2}(1-k')$
$\frac{1}{2}\mathrm{K}+i\mathrm{K}'$	$-\frac{1}{2}\frac{i\pi}{\mathrm{K}}+\frac{1}{2}(1+k')$
$\frac{3}{2}\mathrm{K}+i\mathrm{K}'$	$-\frac{1}{2}\frac{i\pi}{\mathrm{K}}-\frac{1}{2}(1+k')$
$\frac{1}{2}i\mathrm{K}'$	$-\frac{1}{4}\frac{i\pi}{\mathrm{K}}+\frac{1}{2}i(1+k)$
$\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$-\frac{1}{4}\frac{i\pi}{\mathrm{K}}+\frac{1}{2}i(1-k)$
$\frac{3}{2}i\mathrm{K}'$	$-\frac{3}{4}\frac{i\pi}{\mathrm{K}}-\frac{1}{2}i(1+k)$
$\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$-\frac{3}{4}\frac{i\pi}{\mathrm{K}}-\frac{1}{2}i(1-k)$
$\frac{1}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$-\frac{1}{4}\frac{i\pi}{\mathrm{K}}+\frac{1}{2}(k+ik')$
$\frac{3}{2}\mathrm{K}+\frac{1}{2}i\mathrm{K}'$	$-\frac{1}{4}\frac{i\pi}{\mathrm{K}}-\frac{1}{2}(k-ik')$
$\frac{1}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$-\frac{3}{4}\frac{i\pi}{\mathrm{K}}+\frac{1}{2}(k-ik')$
$\frac{3}{2}\mathrm{K}+\frac{3}{2}i\mathrm{K}'$	$-\frac{3}{4}\frac{i\pi}{\mathrm{K}}-\frac{1}{2}(k+ik')$

§ 6. It is convenient also for the sake of completeness to place here the values of the functions for $u=0$, K, iK', &c.

u.	Θu.	Hu.	Zu.
0	$\frac{2^{\frac{1}{3}}k'^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	0	0
K	$\frac{2^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	$\frac{2^{\frac{1}{3}}k^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	0
iK'	0	$iq^{-\frac{1}{4}}\frac{2^{\frac{1}{3}}k'^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	∞
$K+iK'$	$q^{-\frac{1}{4}}\frac{2^{\frac{1}{3}}k^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	$q^{-\frac{1}{4}}\frac{2^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	$-\frac{1}{2}\frac{i\pi}{K}$
2K	$\frac{2^{\frac{1}{3}}k'^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	0	0
$2iK'$	$-q^{-1}\frac{2^{\frac{1}{3}}k'^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	0	$-\frac{i\pi}{K}$*
$2K+iK'$	0	$-iq^{-\frac{1}{4}}\frac{2^{\frac{1}{3}}k'^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	∞
$K+2iK'$	$q^{-1}\frac{2^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	$q^{-1}\frac{2^{\frac{1}{3}}k^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	$-\frac{i\pi}{K}$
$2K+2iK'$	$-q^{-1}\frac{2^{\frac{1}{3}}k'^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}$	0	$-\frac{i\pi}{K}$

§ 7. It follows from the values of Θu and Hu in § 3 that if

$$A'=\frac{2^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}k'^{\frac{1}{3}}(1+k')^{\frac{1}{3}},$$

$$B'=\frac{2^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}k'^{\frac{1}{3}}(1-k')^{\frac{1}{3}},$$

$$A=\frac{2^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}k^{\frac{1}{3}}(1+k)^{\frac{1}{3}},$$

$$B=\frac{2^{\frac{1}{3}}K^{\frac{1}{3}}}{\pi^{\frac{1}{3}}}k^{\frac{1}{3}}(1-k)^{\frac{1}{3}},$$

* The value of $Z(2iK')$ is by mistake given as 0 on page 164 of the "Fundamenta Nova."

then

$$\begin{array}{lll|lll}
\Theta(\frac{1}{2}\mathrm{K})=\Theta(\frac{3}{2}\mathrm{K}) & = & \mathrm{A}' & \mathrm{H}(\frac{1}{2}\mathrm{K})=(\mathrm{H}\frac{3}{2}\mathrm{K}) & = & \mathrm{B}' \\
\Theta(\frac{1}{2}\mathrm{K}+i\mathrm{K}') & = & q^{-\frac{1}{4}}.\mathrm{B}'.\frac{1}{2}(1+i) & \mathrm{H}(\frac{1}{2}\mathrm{K}+i\mathrm{K}') & = & q^{-\frac{1}{4}}.\mathrm{A}'.\frac{1}{2}(1+i) \\
\Theta(\frac{3}{2}\mathrm{K}+i\mathrm{K}') & = & q^{-\frac{1}{4}}.\mathrm{B}'.\frac{1}{2}(1-i) & \mathrm{H}(\frac{3}{2}\mathrm{K}+i\mathrm{K}') & = & q^{-\frac{1}{4}}.\mathrm{A}'.\frac{1}{2}(1-i) \\
\Theta(\frac{1}{2}i\mathrm{K}') & = & q^{-\frac{1}{16}}.\mathrm{B} & \mathrm{H}(\frac{1}{2}i\mathrm{K}') & = & iq^{-\frac{1}{16}}.\mathrm{B} \\
\Theta(\mathrm{K}+\frac{1}{2}i\mathrm{K}') & = & q^{-\frac{1}{16}}.\mathrm{A} & \mathrm{H}(\mathrm{K}+\frac{1}{2}i\mathrm{K}') & = & q^{-\frac{1}{16}}.\mathrm{A} \\
\Theta(\frac{3}{2}i\mathrm{K}') & = & -q^{-\frac{9}{16}}.\mathrm{B} & \mathrm{H}(\frac{3}{2}i\mathrm{K}') & = & iq^{-\frac{9}{16}}.\mathrm{B} \\
\Theta(\mathrm{K}+\frac{3}{2}i\mathrm{K}') & = & q^{-\frac{9}{16}}.\mathrm{A} & \mathrm{H}(\mathrm{K}+\frac{3}{2}i\mathrm{K}') & = & q^{-\frac{9}{16}}.\mathrm{A}
\end{array}$$

Thus

$$\begin{aligned}
\mathrm{A}'=\Theta(\tfrac{1}{2}\mathrm{K})&=\Theta(\tfrac{3}{2}\mathrm{K}) \\
&=q^{\frac{1}{4}}(1-i)\mathrm{H}(\tfrac{1}{2}\mathrm{K}+i\mathrm{K}')= q^{\frac{1}{4}}(1+i)\mathrm{H}(\tfrac{3}{2}\mathrm{K}+i\mathrm{K}') \\
\mathrm{B}'=\mathrm{H}(\tfrac{1}{2}\mathrm{K})&=\mathrm{H}(\tfrac{3}{2}\mathrm{K}) \\
&=q^{\frac{1}{4}}(1-i)\Theta(\tfrac{1}{2}\mathrm{K}+i\mathrm{K}') = q^{\frac{1}{4}}(1+i)\Theta(\tfrac{3}{2}\mathrm{K}+i\mathrm{K}') \\
\mathrm{A}=q^{\frac{1}{16}}\Theta(\mathrm{K}+\tfrac{1}{2}i\mathrm{K}') &= q^{\frac{9}{16}}\Theta(\mathrm{K}+\tfrac{3}{2}i\mathrm{K}') \\
=q^{\frac{1}{16}}\mathrm{H}(\mathrm{K}+\tfrac{1}{2}i\mathrm{K}') &= q^{\frac{9}{16}}\mathrm{H}(\mathrm{K}+\tfrac{3}{2}i\mathrm{K}') \\
\mathrm{B}=q^{\frac{1}{16}}\Theta(\tfrac{1}{2}i\mathrm{K}') &= -q^{\frac{9}{16}}\Theta(\tfrac{3}{2}i\mathrm{K}') \\
=-iq^{\frac{1}{16}}\mathrm{H}(\tfrac{1}{2}i\mathrm{K}') &= -iq^{\frac{9}{16}}\mathrm{H}(\tfrac{3}{2}i\mathrm{K}')
\end{aligned}$$

whence

$$\frac{\mathrm{B}'}{\mathrm{A}'}=\frac{\mathrm{H}(\frac{1}{2}\mathrm{K})}{\Theta(\frac{1}{2}\mathrm{K})}=\frac{\mathrm{H}(\frac{3}{2}\mathrm{K})}{\Theta(\frac{3}{2}\mathrm{K})}=\frac{\Theta(\frac{1}{2}\mathrm{K}+i\mathrm{K}')}{\mathrm{H}(\frac{1}{2}\mathrm{K}+i\mathrm{K}')}=\frac{\Theta(\frac{3}{2}\mathrm{K}+i\mathrm{K}')}{\mathrm{H}(\frac{3}{2}\mathrm{K}+i\mathrm{K}')}$$

$$\frac{\mathrm{B}}{\mathrm{A}}=\frac{\Theta(\frac{1}{2}i\mathrm{K}')}{\Theta(\mathrm{K}+\frac{1}{2}i\mathrm{K}')}=-\frac{\Theta(\frac{3}{2}i\mathrm{K}')}{\Theta(\mathrm{K}+\frac{3}{2}i\mathrm{K}')}=-i\frac{\mathrm{H}(\frac{1}{2}i\mathrm{K}')}{\mathrm{H}(\mathrm{K}+\frac{1}{2}i\mathrm{K}')}=-i\frac{\mathrm{H}(\frac{3}{2}i\mathrm{K}')}{\mathrm{H}(\mathrm{K}+\frac{3}{2}i\mathrm{K}')},$$

equations which admit of immediate verification.

Also, if $$\mathrm{C}=\frac{\mathrm{K}}{\pi}k^{\frac{1}{4}}k'^{\frac{1}{4}},$$

then

$$\begin{aligned}
\Theta(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')\mathrm{H}(\tfrac{1}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')&= q^{-\frac{1}{4}}\mathrm{C}(1+i), \\
\Theta(\tfrac{3}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')\mathrm{H}(\tfrac{3}{2}\mathrm{K}+\tfrac{1}{2}i\mathrm{K}')&= q^{-\frac{1}{4}}\mathrm{C}(1-i), \\
\Theta(\tfrac{1}{2}\mathrm{K}+\tfrac{3}{2}i\mathrm{K}')\mathrm{H}(\tfrac{1}{2}\mathrm{K}+\tfrac{3}{2}i\mathrm{K}')&=-q^{-\frac{1}{4}}\mathrm{C}(1-i), \\
\Theta(\tfrac{3}{2}\mathrm{K}+\tfrac{3}{2}i\mathrm{K}')\mathrm{H}(\tfrac{3}{2}\mathrm{K}+\tfrac{3}{2}i\mathrm{K}')&=-q^{-\frac{1}{4}}\mathrm{C}(1+i).
\end{aligned}$$

§ 8. The q-series for Θ and H are

$$\Theta u=1-2q\cos\frac{\pi u}{\mathrm{K}}+2q^4\cos\frac{2\pi u}{\mathrm{K}}-2q^9\cos\frac{3\pi u}{\mathrm{K}}+\ \&\mathrm{c.},$$

$$\mathrm{H}u=2q^{\frac{1}{4}}\sin\frac{\pi u}{2\mathrm{K}}-2q^{\frac{9}{4}}\sin\frac{3\pi u}{2\mathrm{K}}+2q^{\frac{25}{4}}\sin\frac{5\pi u}{\mathrm{K}}-\ \&\mathrm{c.},$$

and comparing the q-series for $\Theta(\frac{1}{2}\mathrm{K})$, &c., with their values A′, &c., given at the beginning of § 7, we have the following equations :—

$$1-2q^4+2q^{16}-2q^{36}+\ \&c. =\frac{2^{\frac{1}{2}}K^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k'^{\frac{1}{8}}(1+k')^{\frac{1}{4}} \quad . \quad . \quad . \quad (3),$$

$$q^{\frac{1}{4}}-q^{\frac{9}{4}}-q^{\frac{25}{4}}+q^{\frac{49}{4}}+q^{\frac{81}{4}}-\ \&c. =\frac{K^{\frac{1}{2}}}{2^{\frac{1}{2}}\pi^{\frac{1}{2}}}k'^{\frac{1}{8}}(1-k')^{\frac{1}{4}} \quad . \quad . \quad . \quad (4),$$

$$q^{\frac{1}{16}}+q^{\frac{9}{16}}+q^{\frac{25}{16}}+q^{\frac{49}{16}}+q^{\frac{81}{16}}+\ \&c. =\frac{2^{\frac{1}{2}}K^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{8}}(1+k)^{\frac{1}{4}} \quad . \quad . \quad . \quad (5),$$

$$q^{\frac{1}{16}}-q^{\frac{9}{16}}-q^{\frac{25}{16}}+q^{\frac{49}{16}}+q^{\frac{81}{16}}-\ \&c. =\frac{2^{\frac{1}{2}}K^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{8}}(1-k)^{\frac{1}{4}} \quad . \quad . \quad . \quad (6),$$

the signs of the terms in (4) and (6) being, after the first, alternately negative and positive in pairs.

Of these (3) is deducible at once from

$$1-2q+2q^4-2q^9+\ \&c. =\Theta 0=\frac{2^{\frac{1}{2}}k'^{\frac{1}{8}}K^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}$$

by the transformation of q into q^2 twice repeated. The same double transformation converts (6) into (4), and (5) into the formula

$$q^{\frac{1}{4}}+q^{\frac{9}{4}}+q^{\frac{25}{4}}+q^{\frac{49}{4}}+\ \&c. =\frac{k^{\frac{1}{4}}K^{\frac{1}{2}}}{2^{\frac{1}{2}}\pi^{\frac{1}{2}}};$$

so that (3), ... (6) may be readily obtained independently of the values of $\Theta(\frac{1}{2}K)$, &c.

If the modulus be changed from k to k', (6) becomes

$$r^{\frac{1}{16}}-r^{\frac{9}{16}}-r^{\frac{25}{16}}+r^{\frac{49}{16}}+r^{\frac{81}{16}}-\ \&c. =\frac{2^{\frac{1}{2}}K'^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k'^{\frac{1}{8}}(1-k')^{\frac{1}{4}},$$

where $r=e^{-\frac{\pi K}{K'}}$.

Thus $$\frac{q^{\frac{1}{4}}-q^{\frac{9}{4}}-q^{\frac{25}{4}}+q^{\frac{49}{4}}+q^{\frac{81}{4}}-\ \&c.}{r^{\frac{1}{16}}-r^{\frac{9}{16}}-r^{\frac{25}{16}}+r^{\frac{49}{16}}+r^{\frac{81}{16}}-\ \&c.}=\frac{1}{2^{\frac{1}{2}}}\left(\frac{K}{K'}\right)^{\frac{1}{2}},$$

so that if q, r be any quantities less than unity connected by the relation

$$\log\frac{1}{q}\log\frac{1}{r}=\pi^2,$$

then $$\left(\log\frac{1}{q}\right)^{\frac{1}{4}}(q^{\frac{1}{4}}-q^{\frac{9}{4}}-q^{\frac{25}{4}}+q^{\frac{49}{4}}+q^{\frac{81}{4}}-\ \&c.)$$

$$=\frac{1}{2^{\frac{1}{2}}}\left(\log\frac{1}{r}\right)^{\frac{1}{4}}(r^{\frac{1}{16}}-r^{\frac{9}{16}}-r^{\frac{25}{16}}+r^{\frac{49}{16}}+r^{\frac{81}{16}}-\ \&c.),$$

which may also be written

$$e^{-a^2}-e^{-9a^2}-e^{-25a^2}+e^{-49a^2}+\ \&c.$$

$$=\frac{\pi^{\frac{1}{2}}}{2^{\frac{3}{2}}a}(e^{-\frac{1}{64}\frac{\pi^2}{a^2}}-e^{-\frac{9}{64}\frac{\pi^2}{a^2}}-e^{-\frac{25}{64}\frac{\pi^2}{a^2}}+e^{-\frac{49}{64}\frac{\pi^2}{a^2}}+\ \&c.),$$

or $$\mu^{\frac{1}{2}}(e^{-\frac{1}{8}\mu}-e^{-\frac{9}{8}\mu}-e^{-\frac{25}{8}\mu}+e^{-\frac{49}{8}\mu}+\text{ \&c.})$$
$$=\nu^{\frac{1}{2}}(e^{-\frac{1}{8}\nu}-e^{-\frac{9}{8}\nu}-e^{-\frac{25}{8}\nu}+e^{-\frac{49}{8}\nu}+\text{ \&c.}) \quad . \quad . \quad . \quad . \quad (7),$$

where μ, ν are any quantities connected by the relation

$$\mu\nu=\pi^2.$$

Changing q into q^2, (6) becomes

$$q^{\frac{1}{8}}-q^{\frac{9}{8}}-q^{\frac{25}{8}}+q^{\frac{49}{8}}+\text{ \&c.}=\frac{K^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{4}}k'^{\frac{1}{4}},$$

which also leads to (7). This identity is of the same kind as the well-known formula

$$\mu^{\frac{1}{2}}(1+2e^{-\mu}+2e^{-4\mu}+2e^{-9\mu}+\text{ \&c.})=\nu^{\frac{1}{2}}(1+2e^{-\nu}+2e^{-4\nu}+2e^{-9\nu}+\text{ \&c.}),$$

viz., in which a function of μ is equated to the same function of $\frac{\pi^2}{\mu}$. Identities of this class form the subject of a paper in the fifth volume of the "Messenger of Mathematics,"* pp. 174—179; and the identity which results from (3) and (5), viz.:

$$2\mu^{\frac{1}{2}}(e^{-\frac{1}{4}\mu}+e^{-\frac{9}{4}\mu}+e^{-\frac{25}{4}\mu}+\text{ \&c.})=\nu^{\frac{1}{2}}(1-2e^{-\nu}+2e^{-4\nu}-2e^{-9\nu}+\text{ \&c.})$$

is given on page 177 of that paper.

§ 9. From the third group of values in § 3 it might be shown that

$$q^{\frac{1}{16}}-q^{\frac{49}{16}}-q^{\frac{81}{16}}+q^{\frac{225}{16}}+q^{\frac{289}{16}}-\text{ \&c.}=\frac{2^{\frac{1}{2}}K^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{4}}k'^{\frac{1}{4}}\cos\tfrac{1}{4}\theta \quad . \quad . \quad (8),$$

$$q^{\frac{9}{16}}-q^{\frac{25}{16}}-q^{\frac{121}{16}}+q^{\frac{169}{16}}+q^{\frac{361}{16}}-\text{ \&c.}=\frac{2^{\frac{1}{2}}K^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{4}}k'^{\frac{1}{4}}\sin\tfrac{1}{4}\theta \quad . \quad . \quad (9),$$

where θ denotes the modular angle. We thus have

$$\tan\tfrac{1}{4}\theta=\frac{q^{\frac{9}{16}}-q^{\frac{25}{16}}-q^{\frac{121}{16}}+q^{\frac{169}{16}}+q^{\frac{361}{16}}-\text{ \&c.}}{q^{\frac{1}{16}}-q^{\frac{49}{16}}-q^{\frac{81}{16}}+q^{\frac{225}{16}}+q^{\frac{289}{16}}-\text{ \&c.}}.$$

It is a known theorem† that

$$\tfrac{1}{4}\theta=\arctan q^{\frac{1}{3}}-\arctan q^{\frac{3}{3}}+\arctan q^{\frac{5}{3}}-\text{ \&c.},$$

so that
$$\arctan q^{\frac{1}{3}}-\arctan q^{\frac{3}{3}}+\arctan q^{\frac{5}{3}}-\text{ \&c.}$$
$$=\arctan\frac{q^{\frac{9}{16}}-q^{\frac{25}{16}}-q^{\frac{121}{16}}+q^{\frac{169}{16}}+q^{\frac{361}{16}}-\text{ \&c.}}{q^{\frac{1}{16}}-q^{\frac{49}{16}}-q^{\frac{81}{16}}+q^{\frac{225}{16}}+q^{\frac{289}{16}}-\text{ \&c.}}.$$

In the "Philosophical Magazine"‡ for December, 1875, it is shown that

* "Notes on certain Formulæ in Jacobi's 'Fundamenta Nova'" (March and April, 1876).

† "Fundamenta Nova," p. 108.

‡ "On some Identities derived from Elliptic-Function Formulæ," ser. 4, vol. 50, pp. 539—542.

$$\cos(\arctan q - \arctan q^3 + \arctan q^5 - \&c.)$$

$$= \frac{1-q^6-q^{10}+q^{28}+q^{36}-\&c.}{(1+q^2)^{\frac{1}{2}}(1+q^6)^{\frac{1}{2}}\ldots 1-q^4 \,.\, 1-q^8\ldots} \quad . \quad . \quad . \quad (10),$$

$$\sin(\arctan q - \arctan q^3 + \arctan q^5 - \&c.)$$

$$= \frac{q-q^3-q^{15}+q^{21}+q^{45}-\&c.}{(1+q^2)^{\frac{1}{2}}(1+q^6)^{\frac{1}{2}}\ldots 1-q^4 \,.\, 1-q^8\ldots} \quad . \quad . \quad . \quad (11),$$

the exponents being the triangular numbers. These formulæ can be readily identified with (8) and (9); for

$$(1+q \,.\, 1+q^3\ldots)^{\frac{1}{2}} = \frac{2^{\frac{1}{12}}q^{\frac{1}{48}}}{k^{\frac{1}{24}}k'^{\frac{1}{24}}},$$

$$(1-q^2 \,.\, 1-q^4\ldots) = \frac{2^{\frac{1}{6}}\mathrm{K}^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}k^{\frac{1}{6}}k'^{\frac{1}{6}}q^{-\frac{1}{12}},$$

so that, replacing q by $q^{\frac{1}{2}}$ and putting for

$$\arctan q^{\frac{1}{2}} - \arctan q^{\frac{3}{2}} + \arctan q^{\frac{5}{2}} - \&c.,$$

its value $\frac{1}{4}\theta$, (10) and (11) become

$$\cos\tfrac{1}{4}\theta = \frac{\pi^{\frac{1}{2}}}{2^{\frac{1}{3}}\mathrm{K}^{\frac{1}{2}}}\frac{q^{\frac{1}{16}}}{k^{\frac{1}{3}}k'^{\frac{1}{3}}}(1-q^3-q^5+q^{14}+q^{18}-\&c.),$$

$$\sin\tfrac{1}{4}\theta = \frac{\pi^{\frac{1}{2}}}{2^{\frac{1}{3}}\mathrm{K}^{\frac{1}{2}}}\frac{q^{\frac{1}{16}}}{k^{\frac{1}{3}}k'^{\frac{1}{3}}}(q^{\frac{1}{2}}-q^{\frac{3}{2}}-q^{\frac{15}{2}}+q^{\frac{21}{2}}+q^{\frac{45}{2}}-\&c.),$$

which are in fact (8) and (9).

X. "On certain Definite Integrals." No. 5. By W. H. L. Russell, F.R.S. Received October 13, 1879.

The following is a continuation of four papers inserted in the "Proceedings of the Royal Society." In my last paper I gave a proof that

$$\sin(2n+3)\theta - \sin(2n+1)\theta = 0$$

when (n) is infinite. We may consider the subject also thus. Taking the expression

$$\frac{(2n+3)\theta - \frac{(2n+3)^3}{1\,.\,2\,.\,3}\theta^3 + \ldots \pm \frac{(2n+3)^{2m+1}}{1\,.\,2\ldots 2m+1}\theta^{2m+1}}{(2n+1)\theta - \frac{(2n+1)^3}{1\,.\,2\,.\,3}\theta^3 \quad \ldots \pm \frac{(2n+1)^{2m+1}}{1\,.\,2\,.\,3\,.\,2m+1}\theta^{2m+1}}$$

we see manifestly that as (n) increases to infinity the fraction becomes unity. And this is true however large (m) is considered to be.

Manifestly therefore when (n) is infinite $\frac{\sin(2n+3)\theta}{\sin(2n+1)\theta}=1$, another form of the same theorem.

Moreover $\alpha\sin\theta+\alpha^3\sin 3\theta+\ldots=\frac{(\alpha+\alpha^3)\sin\theta}{1-2\alpha^2\cos 2\theta+\alpha^4}$ when α is less than unity.

As α approaches unity in the limit,

$$\sin\theta+\sin 3\theta+\sin 5\theta\ldots=\frac{1}{2\sin\theta},$$

a well-known theorem.

But also

$$\sin\theta+\sin 3\theta+\ldots\sin(2n+1)\theta=\frac{2\sin\theta-\sin(2n+3)\theta+\sin(2n+1)\theta}{2(1-\cos 2\theta)}.$$

Making (n) infinite and subtracting the last equation we have again $\sin(2n+3)\theta-\sin(2n+1)\theta=0$.

I have considered this subject at length on account of a remark of Professor Stokes, that $\sin(2n+3)\theta-\sin(2n+1)\theta$ cannot converge to zero in the ordinary manner, on account of the perpetual fluctuation of the signs of $\sin(2n+3)\theta$ and $\sin(2n+1)\theta$. There can only I think be one explanation of this, namely, that the sine of an infinite angle is zero. Following up then the theorem we find

$$(86.)\quad \int_0^\pi \frac{d\theta}{\sin\theta}\epsilon^{\alpha\cos\theta}\sin(\alpha\sin\theta)=\frac{\pi}{2}(\epsilon^{\alpha}-\epsilon^{-\alpha}).$$

$$(87.)\quad \int_0^\pi \frac{d\theta}{\sin\theta}\cos\alpha\sin\theta(\epsilon^{\alpha\sin\theta}-\epsilon^{-\alpha\sin\theta})=2\pi\sin\alpha.$$

$$(88.)\quad \int_0^\pi \frac{d\theta\sin(2n+1)\theta}{\sin\theta}=\pi.$$

We shall also obtain the following:—

$$(89.)\quad \int_0^\pi d\theta\epsilon^{x\cos\theta}\cos(x\sin\theta+\theta)\log_\epsilon(1+2\alpha\cos\theta+\alpha^2)=\frac{\pi(\epsilon^{\alpha x}-1)}{x\epsilon^{\alpha x}},$$

$$(90.)\quad \int_0^\pi d\theta\,.\,\frac{\epsilon^{x\cos\theta}\cos(x\sin\theta-\theta)+\cos\theta}{\epsilon^{2x\cos\theta}+2\epsilon^{x\cos\theta}\cos(x\sin\theta)+1}\log_\epsilon(1-2\alpha\cos\theta+\alpha^2)$$
$$=\frac{\pi}{x}\log_\epsilon\frac{\epsilon^{\alpha x}+1}{2\epsilon^{\alpha x}}.$$

$$(91.)\quad \int_0^\pi d\theta\,.\,\frac{(1+\alpha^2)\cos\theta+2\alpha}{(1+2\alpha\cos\theta+\alpha^2)^2}\log_\epsilon(1+2\alpha\cos\theta+\alpha^2)=\frac{2\pi\alpha}{1-\alpha^2}.$$

Since $$\frac{x^2}{1.2}+\frac{x^3}{2.3}+\frac{x^4}{3.4}+\ldots=x+(1-x)\log_\epsilon(1-x),$$

when x is any quantity from 0 to 1, inclusive, we shall have

$$\frac{\cos 2\theta}{1.2}+\frac{\cos 3\theta}{2.3}+\frac{\cos 4\theta}{3.4}+\ldots=\cos\theta+2\sin^2\frac{\theta}{2}\log_\epsilon\left(2\sin\frac{\theta}{2}\right)-\frac{\pi-\theta}{2}\sin\theta.$$

We shall call this expression Θ, and shall write more generally

$$\Theta^{(r)}_{m,n}=\frac{\cos r\theta}{mn}+\frac{\cos(r+1)\theta}{(m+1)(n+1)}+\ldots,$$

then it is manifest that $\Theta^{(3)}_{2,3}$, $\Theta^{(4)}_{3,4}$, ... may be found immediately in terms of Θ.

Again, since

$$\frac{x^3}{1.3}+\frac{x^4}{2.4}+\frac{x^5}{3.5}+=\frac{x}{2}+\frac{x^2}{4}+\frac{1-x^2}{2}\log_\epsilon(1-x),$$

we may find a precisely similar expression for $\Theta^{(3)}_{1,3}$ and consequently for $\Theta^{(4)}_{2,4}$, $\Theta^{(5)}_{3,5}$ ...

We thus obtain

(92.) $$\int_0^\pi \Theta\cos r\theta\,.\,d\theta=\frac{\pi}{2(r+1)r}.$$

(93.) $$\int_0^\pi \Theta d\theta\,.\,\frac{\alpha^2\cos 2\theta-\alpha^3\cos\theta}{1-2\alpha\cos\theta+\alpha^2}=\frac{\pi}{2}\{\alpha+(1-\alpha)\log_\epsilon(1-\alpha)\}.$$

(94.) $$\int_0^\pi \Theta d\theta\epsilon^{\alpha\cos\theta}\cos(\alpha\sin\theta+2\theta)=\frac{\pi}{2}\left\{\frac{\epsilon^\alpha}{\alpha^2}-\frac{1+\alpha}{\alpha^2}\right\}.$$

(95.) $$\int_0^\pi \Theta^{(3)}_{12}d\theta\{\log_\epsilon(1+2x\cos\theta+x^2)+2x\cos\theta+x^2\cos 2\theta\}$$
$$=\frac{\pi}{2}\{(1-x)^2\log_\epsilon(1-x)-(x^2+x)\}.$$

(96.) $$\int_0^\pi d\theta\Theta^{(3)}_{23}\epsilon^{\alpha\cos\theta}\cos(\alpha\sin\theta+3\theta)=\frac{\pi}{2\alpha^3}\{\epsilon^\alpha(\alpha-2)+(\alpha+2)\}.$$

(97.) $$\int_0^\pi d\theta\Theta^{(4)}_{2,4}\epsilon^{\alpha\cos\theta}\cos(\alpha\sin\theta+4\theta)=\frac{\pi}{2\alpha^4}\left\{\epsilon^\alpha(\alpha^2-3\alpha+3)+\frac{\alpha^2}{2}-3\right\}.$$

(98.) $$\int_0^\pi d\theta\,.\,\Theta^{(4)}_{12}\Theta^{(4)}_{34}=\frac{\pi}{36}.$$ (99.) $$\int_0^\pi d\theta\Theta^{(5)}_{12}\Theta^{5}_{45}=\frac{5\pi}{288}.$$

More generally we may find

(100.) $$\int_0^\pi d\theta\Theta^{(s)}_{m,n}\Theta^{(s)}_{r,s},$$

where m, n, r, s, are in order of magnitude.

The instance I have mentioned at the commencement of this paper, is not the only instance of my obligation to the kindness of Professor Stokes. He prevented me on another occasion from being led to a false conclusion, by taking for granted without due examination an integral which had appeared in print.

XI. "On the Normal Paraffins. Part III." By C. SCHORLEMMER, F.R.S., Professor of Organic Chemistry in Owens College, Manchester. Received August 2, 1879.

(Abstract.)

The isomeric monochlorides, obtained from the normal paraffins existing in petroleum, yield by the abstraction of hydrochloric acid a mixture of olefines, one portion of which readily combines with hydrochloric acid in the cold, whilst the other unites with it only on heating.*

The chlorides formed in the cold boil with partial decomposition and at a lower temperature than the others, which distil without undergoing any change, and have the general formula

$$CH_3—CHCl—C_nH_{2n+1}.$$†

They are therefore derived from the olefines of the series

$$CH_2{=}CH—C_nH_{2n+1}.$$

Similar results have been obtained by Le Bel.‡

The constitution of the olefines combining with the acid in the cold is not yet known. They are possibly not derived from normal paraffins, but from isomerides, which cannot be separated from the former by distillation. On the other hand their formation can also be explained without making this assumption. This question can only be decided by using an absolutely pure paraffin.§

For this purpose, normal hexane from mannite was selected, which possibly might yield three isomeric monochlorides:

(1.) $CH_3—CH_2—CH_2—CH_2—CH_2—CH_2Cl$
(2.) $CH_3—CH_2—CH_2—CH_2—CHCl—CH_3$
(3.) $CH_3—CH_2—CH_2—CHCl—CH_2—CH_3$

The formation of the first two has already been proved.‖ The following seemed capable of determining whether the third is also produced. By the abstraction of hydrochloric acid three hexylenes might be formed:—

Butylethylene	$CH_3—CH_2—CH_2—CH_2—CH{=}CH_2$
Methylpropylethylene ..	$CH_3—CH_2—CH_2—CH{=}CH—CH_3$
Diethylethylene	$CH_3—CH_2—CH{=}CH—CH_2—CH_3$

The first of these does not combine with cold hydrochloric acid; the second is the hexylene obtained from secondary hexyl iodide.‖

* "Journ. Chem. Soc.," 1873, p. 319.
† Morgan, *ibid.*, 1875, p. 301.
‡ "Bull. Soc. Chim.," (2), xxviii, p. 460.
§ "Journ. Chem. Soc.," 1875, p. 306.
‖ Hecht, "Deut. Chem. Ges. Ber.," xi, p. 1152.

Le Bel and Wassermann have found that cold hydrochloric acid has no action on it, from which it follows that, if normal hexane from mannite yields a hexylene combining with hydrochloric acid in the cold, it could be only diethylhexylene, which could be easily identified by conversion into ethylpropyl carbinol and oxidising it, when only propionic acid should be formed.

This was my programme; the results were, however, quite unexpected.

The hexylene obtained by decomposing the hexyl chlorides was left in contact with cold fuming hydrochloric acid for some weeks. *The whole of it combined and the hexyl chloride thus formed boiled constantly and without the least decomposition* at 124—125°. It was converted into the alcohol, which on oxidation yielded only acetic acid and butyric acid, and consequently is methylbutyl carbinol. We have, therefore, the remarkable fact, that two hexanes exist, which must be regarded as normal compounds, and therefore according to our present theory, to be identical. This is, however, not the case. I have already in my first paper pointed out some other differences existing between the two hexanes, but left the question open, whether these are caused by impurities contained in the hexane from petroleum, or whether we have here a case of fine isomerism, for which an explanation has to be found.* I believe the results of my present research speak strongly in favour of the latter view.

For several reasons I am inclined to believe that petroleum consists chiefly of an inextricable mixture of isomeric and homologous paraffins, in which, however, the normal hydrocarbons preponderate. This would certainly explain why it is so difficult to isolate from it bodies having a constant boiling point,† but not the differences exhibited by the two hexanes.

A continuation of these researches has already been commenced. My friend Thorpe, who has made the most interesting discovery that the terebinthinate exudation of *Pinus Sabiniana* contains a large quantity of normal heptane,‡ has kindly offered me to join him in the chemical investigation of this hydrocarbon. At the same time we shall compare it with other "normal" heptanes from different sources.

XII. "The Geometric Mean, in Vital and Social Statistics." By Francis Galton, F.R.S. Received October 21, 1879.

My purpose is to show that an assumption which lies at the basis of the well-known law of "Frequency of Error" (commonly expressed

* "Phil. Trans.," vol. clxii, p. 119.

† "Journ. Chem. Soc.," 1875, p. 306.

‡ "Journ. Chem. Soc.," 1879, p. 296.

by the formula $y=e^{-h^2x^2}$), is incorrect in many groups of vital and social phenomena, although that law has been applied to them by statisticians with partial success and corresponding convenience. Next, I will point out the correct hypothesis upon which a Law of Error suitable to these cases ought to be calculated; and subsequently I will communicate a memoir by Mr. Donald McAlister, who, at my suggestion, has mathematically investigated the subject.

The assumption to which I refer is, that errors in excess or in deficiency of the truth are equally probable; or conversely, that if two fallible measurements have been made of the same object, their arithmetical mean is more likely to be the true measurement than any other quantity that can be named.

This assumption cannot be justified in vital phenomena. For example, suppose we endeavour to match a tint; Fechner's law, in its approximative and simplest form of sensation=log stimulus, tells us that a series of tints, in which the quantities of white scattered on a black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to the eye to be separated by equal intervals of tint. Therefore, in matching a grey that contains 8 portions of white, we are just as likely to err by selecting one that has 16 portions as one that has 4 portions. In the first case there would be an error in excess, of 8; in the second there would be an error in deficiency, of 4. Therefore, an error of the same magnitude in excess or in deficiency is not equally probable in the judgment of tints by the eye. Conversely, if two persons, who are equally good judges, describe their impressions of a certain tint, and one says that it contains 4 portions of white and the other that it contains 16 portions, the most reasonable conclusion is that it really contains 8 portions. The arithmetic mean of the estimates is $\frac{4+16}{2}$ or 10, which is not the most probable value. It is the geometric mean 8 (4 : 8 :: 8 : 16) which is the most probable.

Precisely the same condition characterises every determination by any of the senses; for example, in judging of the weight of bodies and of their temperatures, of the loudness and of the pitch of tones, and of estimates of lengths and distances *as wholes*. Thus, three rods of the lengths a, b, c, when taken successively in the hand, appear to differ by equal intervals when $a : b :: b : c$, and not when $a-b=b-c$. In all physiological phenomena, where there is on the one hand a stimulus and on the other a response to that stimulus, Fechner's law may be assumed to prevail; in other words, the true mean is the geometric.

The same condition of the geometric mean appears to characterise the majority of the influences, which, combined with those of purely vital phenomena, give rise to the events with which sociology deals. It is difficult to find terms sufficiently general to apply to the varied topics of sociology, but there are two categories of causes, which are

of common occurrence. The one is that of ordinary increase, as exemplified by the growth of population, where an already large nation tends to become larger than a small one under similar circumstances, or when a capital employed in a business increases in proportion to its size. The other category is that of surrounding influences, or "milieux" as they are often called, such as a period of plenty in which a larger field or a larger business yields a greater excess over its mean yield than a smaller one. Most of the causes of those differences with which sociology are concerned, and which are not purely vital phenomena, such as those already discussed, may be classified under one or other of these two categories, or under such as are in principle almost the same. In short, sociological phenomena, like vital phenomena are, as a general rule, subject to the condition of the geometric mean.

The ordinary law of Frequency of Error, based on the arithmetic mean, corresponds, no doubt, sufficiently well with the observed facts of vital and social phenomena, to be very serviceable to statisticians, but it is far from satisfying their wants, and it may lead to absurdity when applied to wide deviations. It asserts that deviations in excess must be balanced by deviations of equal magnitude in deficiency therefore, if the former be greater than the mean itself, the latter must be less than zero, that is, must be negative. This is an impossibility in many cases, to which the law is nevertheless applied by statisticians with no small success, so long as they are content to confine its application within a narrow range of deviation. Thus, in respect of stature, the law is very correct in respect to ordinary measurements, although it asserts that the existence of giants, whose height is more than double the mean height of their race, implies the possibility of the existence of dwarfs, whose stature is less than nothing at all.

It is, therefore, an object not only of theoretical interest but of practical use, to thoroughly investigate a Law of Error, based on the geometric mean, even though some of the expected results may perhaps be apparent at first sight. With this view I placed the foregoing remarks in Mr. Donald McAlister's hands, who contributes the following memoir.

XIII. "The Law of the Geometric Mean." By Donald McAlister, B.A., B.Sc., Fellow of St. John's College, Cambridge. Communicated by Francis Galton, F.R.S. Received October 21, 1879.

Suppose we have before us a large number of measurements. They may either be all approximations to the true value of a single unknown quantity, or may refer to the several members of a large class. The

measurements will disagree among themselves, but on arranging them in order of size they show a tendency to cluster round some *medium* value. We are naturally inclined to infer that the true value of the unknown, or typical member of the class, is not far from this value. How to define and determine the appropriate *medium* in various classes of measurement becomes thus a natural object of inquiry. On examination we find that there is no strict and final criterion applicable to all cases. We have to start with an empirical assumption, more or less justifiable on general grounds, but not capable of rigid proof. In the ordinary Theory of Errors, which deals primarily with discrepant *observations*, the assumption made is reducible to this:—that, on the whole, the best *medium* we can take is the Arithmetic Mean of the discordant measures. This is equivalent to the statement that errors (or *differences* from the truth) of equal amount in excess or defect are equally likely to occur. In the class of measures referred to by Mr. Galton, which are *not* of the nature of instrumental observations, reason is given for thinking that, on the whole, a better *medium* value is likely to be obtained by taking the Geometric Mean of the discrepant measures. This is equivalent to the assumption that measures are equally likely which bear to the truth *ratios* of equal amount in excess or defect, so to speak. For example, that measures which are respectively $(\frac{1001}{1000})$th and $(\frac{1000}{1001})$th of the truth are equally likely to occur. This paper seeks to develop some consequences of this fundamental principle.

The practical outcome is a method for the treatment of a series of measures which naturally group themselves round their Geometric Mean. This method may be briefly presented as follows:—

Let the measures be

$$x_1, x_2, x_3 \quad . \quad . \quad . \quad . \quad x_n.$$

Take the (hyperbolic) logarithm of each measure, thus forming a new series, say

$$y_1, y_2, y_3 \quad . \quad . \quad . \quad y_n; \text{ where } y_r = \log_e x_r.$$

Form the Arithmetic Mean (A.M.) of the x's, so that

$$n(\text{A.M.}) = \Sigma x.$$

Form the Arithmetic Mean of the y's: this will be the logarithm of the Geometric Mean (G.M.) of the x's, so that

$$n \log (\text{G.M.}) = \Sigma y.$$

If then $\frac{1}{h^2} \equiv 4 (\log (\text{A.M.}) - \log (\text{G.M.}))$, we have in h a "measure of precision" of the series of x's which we call the "*weight*."

The series of y's *may then be treated as a series grouped round its*

Arithmetic Mean, and the formulæ of the ordinary theory are at once applicable to it, the weight ascribed to it being h, *as above defined.*

When the y-series is so treated, we can at once pass from it to the x-series. Thus the probability, frequency, &c., of any x are the same as for the corresponding y: and any term interpolated in the y-series will be the logarithm of the corresponding term in the x-series.

1. If it be granted that the Geometric Mean of the measures is the *most* probable value of the quantity measured, it follows that measures which differ from the mean have a probability which becomes less as the discrepancy becomes greater, either in excess or defect. We naturally seek for some scale which shall define the respective probability to be assigned to each measure of any given series.

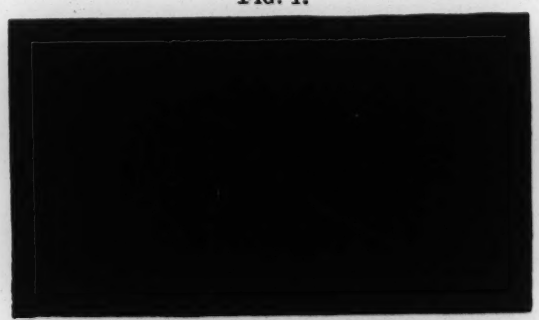
Thus, if x be one of the measures, and a be the Geometric Mean of the whole series, and if we can assign a form to the function ϕ, such that $\phi\left(\frac{x}{a}\right)$ is the probability of x, the curve $y=\phi\left(\frac{x}{a}\right)$ will afford us (graphically) the required scale, and $\phi\left(\frac{x}{a}\right)$ will be the "*Law of Frequency.*"

2. By a method analogous to the first method of Gauss ("Theoria Motus," ii, 3, 186), it can be shown that

$$\phi\left(\frac{x}{a}\right) \equiv \mathrm{B} \exp.\left(-h^2 \log^2 \frac{x}{a}\right) \equiv \mathrm{B} e^{-h^2\left(\log \frac{x}{a}\right)^2},$$

where B and h are constants. B is determined from the condition that the sum of the probabilities of all the measures must be unity; h depends on the grouping of the measures, being large when they agree closely and small when they are generally discordant; h is thus taken as a measure of the precision of the series in general, and is briefly called the "*weight*" *of the series*, or sometimes the "*weight*" *of the mean* derived therefrom. (See fig. 1.)

FIG. 1.



$$y = \mathrm{B} \exp.\left(-h^2 \log^2 \frac{x}{a}\right).$$

3. In forming a scale of probability, $\phi\left(\frac{x}{a}\right)$, which shall be appli-

cable to any series of measures of which the mean and the weight are known, we must suppose the value of ϕ to be given (either graphically or by table) for all possible values of x. In other words, we must suppose the number of entries to be indefinitely great and indefinitely close together. It is clear that the actual probability of any individual measure becomes in such a series infinitesimal, though the relative magnitudes of any pair of probabilities remain unaltered. This is indicated in the formula of §2 by the constant B becoming infinitesimal. To avoid this inconvenience we are led to modify our original problem, and ask, not "what is the probability of a measure x?" but "what is the probability of a measure lying between the very close limits x and $x+\delta x$?"

4. From the principle indicated above, viz., that on the whole there are as many measures less than the mean as there are greater than the mean, we deduce that the required probability is

$$\frac{h}{\sqrt{\pi}} \exp.\left(-h^2 \log^2 \frac{x}{a}\right) \frac{\delta x}{x}.$$

the constant factor $h \div \sqrt{\pi}$ results from the condition that the sum of all the values of this function from $x=0$ to $x=\infty$ must be unity. And it will be found that

$$\int_0^\infty \frac{h}{\sqrt{\pi}} \exp.\left(-h^2 \log^2 \frac{x}{a}\right) \frac{dx}{x} = 1.$$

as should be the case.

The coefficient of δx, viz.:—$h \exp.\left(-h^2 \log^2 \frac{x}{a}\right) \div \sqrt{\pi} \,.\, x$, I have ventured to distinguish from the previous function by calling it the "*Law of Facility*." This function may be defined in words as "the ratio which the chance that a measure lies within a given small interval bears to the magnitude of the interval.

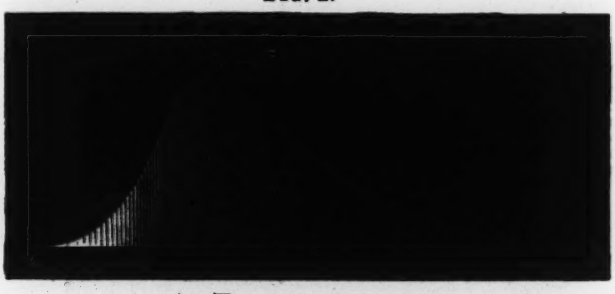
5. Tracing the curve which represents the "Law of Facility," we find that its form resembles that of the curve of frequency, but that the maximum ordinate occurs at $x=a \exp.\left(-\frac{1}{2h^2}\right)$ in the former, and of course at $x=a$ in the latter. The latter result implies that the mean is the most probable value: the former means that there is more chance of a measure lying within a given small fraction (δx) of $a \,.\, \exp.\left(-\frac{1}{2h^2}\right)$ than within the same small fraction of any other value that can be named.

6. We may graphically represent our results in three ways:—

1°. Trace the curve $y\sqrt{\pi} = h \exp.(-h^2 \log^2 x)$, (the mean being taken as unity). Let the space between the curve and Ox be the boundary of a lamina of varying density. Take the density along the ordinate

at x to be $\frac{1}{x}$. Then the ordinate at x is proportional to the (infinitesimal) probability of x, and the mass of the section bounded by the ordinates at x_1, x_2 measures the probability of a measure lying between x_1 and x_2. If we are dealing with a very large number of measures, this mass will be found to represent the number of measures which actually lie between those limits. The mass of the lamina is bisected by the ordinate at $x=1$ (fig. 2).

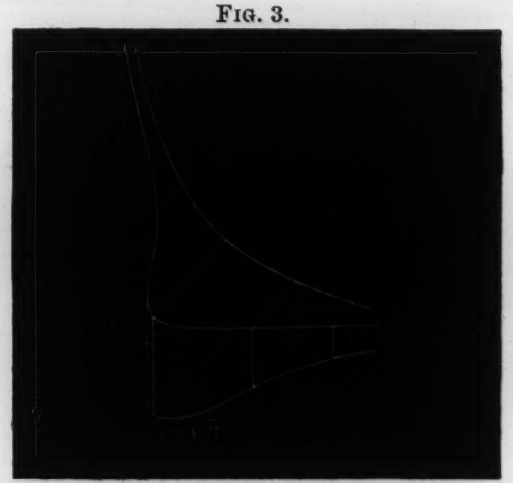
FIG. 2.



$$\begin{cases} y\sqrt{\pi} = h \exp.(-h^2 \log^2 x). \\ \text{density at } x = \frac{1}{x}. \end{cases}$$

2°. Regard the equation $y\sqrt{\pi}=h$ exp. $(-h^2 \log^2 x)$ as that of a cylinder. Let it be cut by another cylinder $zx=1$, at right angles to it. The solid included between these surfaces and the planes xz, xy, obviously corresponds to the lamina of 1°, volume being read for mass (fig. 3). Methods 1° and 2° thus represent in one figure both the frequency and the facility functions.

FIG. 3.



$$\begin{cases} y\sqrt{\pi} = h \exp.(-h^2 \log^2 x) \\ z = \frac{1}{x}. \end{cases}$$

3°. The law of facility alone will be represented by the curve

$$xy\sqrt{\pi}=h\exp.(-h^2\log^2 x).$$

Here the area included between any two ordinates, the curve, and the axis of x, measures the probability of a measure lying between the corresponding abscissæ. The area of the curve is bisected at $x=1$, but the maximum ordinate is at $x=\exp.\left(-\frac{1}{2h^2}\right)$ (fig. 4).

Fig. 4.



$$\begin{cases} xy\sqrt{\pi}=h\exp.(-h^2\log^2 x). \\ \mathrm{OA}=\exp.\left(-\frac{1}{2h^2}\right). \\ \mathrm{AB}=\frac{h}{\sqrt{\pi}}\exp.\left(\frac{1}{2h^2}\right). \end{cases}$$

7. (Quetelet's method.) Suppose the measures we are considering to be a series of estimates of the magnitude of some single object (such as the depth of a given tint). In forming such an estimate the mind is acted on by many small causes tending to make us err either in excess or defect. Suppose each small cause acts in such a way as to make us under- or over-estimate in a fixed ratio; and as a particular case let this fixed ratio be the same for all the small causes. If, then, half the causes tend to over- and half to under-estimation, we estimate rightly. Our estimate in any given case depends on the particular combination of the causes which has (so far as our knowledge is concerned) fortuitously arisen. Let, then, the number of causes be $2n$, the fixed ratio a. The total number of possible combinations will be 2^{2n}. The resulting possible ratios are

$$\alpha^{2n},\ \alpha^{2n-2},\ \ldots\ \alpha^{2},\ 1,\ \alpha^{-2},\ \ldots\ \alpha^{-2n+2},\ \alpha^{-2n}.$$

The chance of a correct estimate (ratio 1) is by the theory of combinations $\underline{|2n} \div 2^{2n}\ \underline{|n}\ \underline{|n}$: the chance of an estimate bearing a ratio $\alpha^{\pm r}$ to the truth will be $\underline{|2n} \div 2^{n}\ \underline{|n-r}\ \underline{|n+r}$. Let now n become very large, and α very near to unity; while r remains finite. Then,

by a known approximation, the chance ultimately takes the form $\frac{1}{\sqrt{n\pi}} \exp.\left(-\frac{r^2}{n}\right)$. If we are asked the chance of a ratio x, we put $x=\alpha^r$, and thus find $\log x = r \log a$; the chance required is then

$$\frac{1}{\sqrt{n\pi}} \exp.\left(-\frac{1}{n}\left(\frac{\log x}{\log \alpha}\right)^2\right),$$

corresponding to the law of frequency.

Next determining the chance that the estimate shall lie between narrow limits x and $x+x'$, we find that we have to multiply the above expression by $x' \div x \log a$. Thus, if we take $h^{-2}=n(\log a)^2$, the expression agrees exactly with that already found for the law of facility. This verification is interesting, for though it depends on certain special suppositions, the process seems to throw light on the genesis of the law, and the significance of the modulus h.

8. Another method of exhibiting the law, suggested by Mr. Galton's Method of Intercomparison, is the following. Let the series of measures be represented by a series of ordinates: arrange these side by side at equal small distances and in order of magnitude. Their extremities will then lie on a curve of contrary flexure, which Mr. Galton calls an Ogive; we may speak of it as the "*curve of distribution.*" Its equation can be shown to be

$$hkx = \int_0^{h \log y} e^{-t^2}\, dt \equiv \text{erf. } (h \log y),$$

k being a constant depending on the interval between the ordinates as they stand (fig. 5).

FIG. 5.



$$\begin{cases} hkx = \text{erf. } (h \log y). \\ \text{OA} = \text{OA}' = \sqrt{\pi} \div 2hk. \\ \text{OB} = \text{OB}' = \frac{1}{2}\text{OA}. \\ \text{BC} = \text{B}'\text{C}' = \frac{1}{4}\text{OA}. \end{cases}$$

9. We may next show that if the Arithmetic Mean (A.M.) of all the measures be formed then

$$\frac{1}{h^2}=4\log\left(\frac{\text{A.M.}}{a}\right).$$

This follows strictly when the number of measures is indefinitely great. It still gives a good approximation to the value of h when the number is considerable.

10. Among the measures greater than the mean there is one which may be called middlemost: *i.e.*, such that it is an even wager that a measure (greater than the mean) lies above it or below it. A similar middlemost measure exists among those that are less than the mean. As these two measures, with the mean, divide the curve of facility into four equal parts, I propose to call them the "*higher quartile*" and the "*lower quartile*" respectively. It will be seen that they correspond to the ill-named "probable errors" of the ordinary theory. If Q, q be the quartiles we can show that

$$h\log\frac{Q}{a}=\cdot4769\ldots=-h\log\frac{q}{a},$$

so that $Qq=a^2$. Thus also

$$Q^h(\cdot6208\ldots)=a^h=q^h(1\cdot611\ldots).$$

Similarly between zero and lower quartile we place a mid-measure which we call the "*lower octile.*" The "*higher octile*" will subdivide the interval between the higher quartile and infinity. If O, o, be the octiles, we have—

$$O^h(\cdot443)=a^h=o^h(2\cdot255\ldots):$$

as before $Oo=a^2$: and

$$\left(\frac{O}{Q}\right)^h=\left(\frac{q}{o}\right)^h=1\cdot40\ldots$$

It will be observed that in the curve of distribution Q and q are ordinates equidistant from the mean and the terminal ordinates. O is equidistant from Q and the asymptote.

The analogues suggested by the mean error, and mean square error, &c., of the ordinary theory have no very practical value or significance for us. It will be remembered that they are introduced to obviate the difficulties arising from *negative* errors. In our problem these somewhat artificial functions have no special place.*

* The mean-square-measure is a exp. $\left(\frac{1}{2h^2}\right)$. The geometric mean of the measures greater than mean is a exp. $\left(\frac{\cdot564\ldots}{h}\right)$.

The gauges of the series given by the weight and the quartiles and octiles will generally suffice.

11. Let two fallible measures be given whose respective weights are known, and let their product be formed. This product will be a fallible measure like its factors. It can be shown that the product is subject to a law of facility of the form already obtained, and further that, as in the ordinary theory, the weight is such that the square of its reciprocal is equal to the sum of the squared reciprocals of the weights of the factors. More generally, if x_1, x_2 . . . be a series of measures whose weights are h_1, h_2 . . . , and if z be connected with them by the equation

$$z = x_1^{n_1} \times x_2^{n_2} \times \ldots .,$$

Then h the weight of z is given by

$$\frac{1}{h^2} = \frac{n_1^2}{h_1^2} + \frac{n_2^2}{h_2^2} + \ldots .$$

These results are strict; that which follows is closely approximate and applies to series whose weight is not small.

Let f (a_1, a_2 . . .) be any function of the fallible measures a_1, a_2 . . . , each of which may be the mean of some considerable number of measures, so that the weights are respectively h_1, h_2 . . . Required the weight to be assigned to the value of the function obtained from these fallible values of its variables. We are able to show that if h_f be the weight in question

$$\frac{1}{h_f^2} = \left(\frac{\partial \log f}{\partial \log a_1}\right)^2 \frac{1}{h_1^2} + \left(\frac{\partial \log f}{\partial \log a_2}\right)^2 \frac{1}{h_2^2} + \ldots .$$

12. It only remains to establish the practical method presented in § 1. We have

$$\left.\begin{matrix}\text{Chance of a logarithm} \\ \text{lying between } y \text{ and } y+\partial y\end{matrix}\right\} = \left\{\begin{matrix}\text{Chance of a measure} \\ \text{lying between } x \text{ and } x+\partial x\end{matrix}\right.$$

$$= \frac{h}{\sqrt{\pi}} \exp.\left(-h^2 \log^2 \frac{x}{a}\right) \frac{\partial x}{x} = \frac{h}{\sqrt{\pi}} \exp.\left(-h^2 \overline{y - \log a}^2\right) \partial y.$$

This is the expression of the ordinary law of grouping of the y's round their arithmetic mean ($\log a$).

It follows that, for example, the number of x's between x_r and x_s must be proportional to

$$\frac{h}{\sqrt{\pi}} \int_{y_r}^{y_s} \exp.\left(-h^2 y^2\right) dy :$$

this can be readily evaluated by means of the ordinary tables of the error-function.

Again, still using h for the weight, the "probable-error" of the

y-series corresponds to "quartile" of the x-series. For, by the ordinary theory

$$h\,(\text{probable error}) = \cdot 4769\ldots$$
$$\therefore = h\,(\log Q - \log a)$$
$$\therefore \log a + \text{prob. error} = \log Q.$$

In words—the term which differs from the Arithmetic Mean by probable error in the y-series is the log. of the quartile in the x-series.

We infer that no new tables are necessary for the practical working of our method. We require only the tables of the error-function and of hyperbolic logarithms. Of course common logarithms may be used, if we remember to introduce the appropriate modulus into our formulæ.

In conclusion, I desire to acknowledge my indebtedness to Mr. Galton, not only for the suggestion of the problem I have here attempted to solve, but also for many valuable practical hints in the working.

XIV.

(Received September 19, 1879.)

Colaba, Bombay,
29th August, 1879.

Dear Sir,

I regret that in my paper on the "Diurnal Variations of the Wind and Barometric Pressure," published in vol. xxv, p. 402, of the "Proceedings of the Royal Society," I have inadvertently referred the Bermuda wind observations to civil time instead of to astronomical time.

The correction of this error shows that the variation of the values of the hourly inequalities of the wind components at Bermuda is very much *less* during the hours when the sun is above the horizon than during those when he is below it, especially in the winter half year.

The calculated barometric variations, depending on the wind variations, should also be referred to astronomical instead of to civil time; but the general reasoning of the paper remains the same, leaving the principal portions of the variations (viz., those which have half a day for their period) unaffected.

(Signed) F. CHAMBERS.

The Secretary of the Royal Society.

November 27, 1879.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election was read as follows:—

President.—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.; Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Professor Alexander W. Williamson, Ph.D., LL.D.

Other Members of the Council.—George Busk, V.P.L.S.; Professor Arthur Cayley, LL.D.; Major-General Henry Clerk, R.A.; Edwin Dunkin, F.R.A.S.; Augustus G. Vernon Harcourt, F.C.S.; Sir Joseph Dalton Hooker, C.B., K.C.S.I.; John Whitaker Hulke, F.R.C.S.; Lieut.-General Sir J. Henry Lefroy, C.B.; William Newmarch, Inst. Fr. Corr.; Professor Alfred Newton, M.A., V.P.Z.S.; Professor William Odling, M.B., V.P.C.S.; Sir James Paget, Bart., D.C.L.; William Henry Perkin, Sec. C.S.; Charles William Siemens, D.C.L.; John Simon, C.B., D.C.L.; Professor John Tyndall, D.C.L., LL.D.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Structure of Serous Glands in Rest and Activity." By J. N. LANGLEY, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor MICHAEL FOSTER, M.D., F.R.S. Received October 13, 1879.

The Parotid Gland of the Rabbit.

In young animals portions of this gland are sufficiently thin to allow them to be viewed by transmitted light in their normal state.

With care the gland of a narcotised animal can be stretched out and looked at without injury to the blood circulation. The rapid capillary current continues for many hours shooting around and between the alveoli. Stimulation of the sympathetic nerve in the neck, besides

causing a secretion in the glands, produces the most striking effects on the blood circulation; the current all but ceases.

The resting gland thus viewed shows alveoli, in which the boundaries of the cells are faintly marked out by light lines between the many granules that fill the cells. The granules stretch from lumen to basement membrane. No nuclei are to be seen in the alveoli, nothing but a mass of not very refractive granules with an indistinct network of light lines which mark the separate cells.

The secreting gland similarly viewed strikes the eye at once as being much more transparent; with a low magnifying power, the lobules even when thick, are no longer the dense dark masses of the resting gland. With a Zeiss D or E objective, it is seen that this is caused by the disappearance of the granules at the outer part of the alveoli, the part near the basement membrane. The later the stage of secretory activity, the larger the non-granular clear zone. It is however only in the earlier secretory stages that the clear portion is in the form of a ring in section, later the clear zone dips down at fairly regular intervals towards the lumen. The granules thus acquire a star-like arrangement. This comes about in the following manner:—As secretion goes on, the cells draw away from one another slightly at the lumen, and at their sides contiguous to the lumen (perhaps because they are there being used up): so that the lumen can be more or less distinctly seen with short prolongations running out between the cells; at the same time the granules disappear more rapidly from the interior than from the periphery of the cells, hence the granules become arranged in a clump at the luminar margin, and stretch out from this some little distance along the cell sides.

The rather troublesome method given above is not necessary for the observation of the living condition of the tissue; small pieces cut out and mounted without the addition of fluid preserve for a short time very closely their normal appearance. If to such a specimen any of the ordinary indifferent fluids be added, the granules become more highly refractive, and later a thorough alteration takes place in the cells, the granules originally present to a large extent at any rate disappear and others are formed.

A very satisfactory and economical method of noting the changes which take place in the gland during secretion, is to narcotise an animal and then to snip out small bits from various parts of the gland at different stages of sympathetic stimulation or pilocarpin injection. The alveoli, at first granular throughout, become less and less so, until hardly any granules are to be seen. At this later stage very little saliva can be obtained.

The cells, during the process of losing their granules, become much smaller; the reparation is not sufficient to cover the waste.

I may remark that it does not necessarily happen that every hungry

rabbit has parotid alveoli throughout granular, the secretion is not solely dependent on feeding, but in whatever stage the glands are at the beginning of an experiment, they become less granular after having been made to secrete.

I have said that the changes can be seen either after sympathetic stimulation or after injection of pilocarpin. This is noteworthy, since Heidenhain,* dealing with alcohol-hardened specimens, failed to observe any of the changes after pilocarpin injection, which he could obtain by stimulation of the sympathetic nerve.

The appearances in the gland described above are not observed in alcohol-hardened specimens.

It will be remembered that Heidenhain describes the secreting gland as being, in alcohol specimens, more granular than the resting; this depends on the relative amounts of cell substances soluble and insoluble in alcohol.

Treated with osmic acid and subsequently with alcohol the gland has a markedly different appearance in the resting and active state.

In the resting state the alveolar cells viewed with a Zeiss F have a close-meshed network throughout. The nuclei are fairly apparent, evenly stained, rather compressed, and peripherally placed. With Zeiss D the cells appear densely granular.

In the active state the punctated look caused by a network and interfibrillar substance of differing staining power is absent, the cells stain fairly evenly throughout, thus the nuclei are with difficulty seen; they are spherical, and are placed more in the middle of the cell.

The Parotid Gland in the Dog, Cat, and Rat.

In these animals the account given above of the resting and active states in the main holds. A more prolonged secreting activity is, however, necessary to produce the clear zone and star arrangement of the granules.

In a former paper† I have mentioned that when just sufficient atropin to paralyse the secretory nerves is injected into the blood, the paralysing effect can be antagonised and a slight secretion obtained many times in succession by injecting small quantities of warm pilocarpin into the duct. I was curious to observe if the secretion obtained by repeatedly antagonising the atropin effect would produce the secretory change. The experiment was tried on a cat, and with a positive result, the granules at the periphery of the alveoli disappeared, a clear zone was formed.

In the dog few observers have seen any saliva follow stimulation of the sympathetic nerve. Heidenhain‡ obtained only now and then a

* Pflüger. "Arch. f. d. Ges. Physiologie," Bd. xvii, p. 45, 1878.

† Foster. "Journal of Physiology," vol. i, p. 356, 1878.

‡ *Op. cit.*, p. 29.

drop or two; he found, nevertheless, that the stimulation caused a marked change in the histological characters of the gland. This I have tried in one dog only; then stimulation of the sympathetic a little below the superior cervical ganglion caused a flow of saliva, slow indeed, but sufficiently distinct. Stimulation about one minute out of every two to three minutes gave in three hours 1·3 cub. centims. of saliva. This is not a great quantity, but the quality of the saliva was most remarkable, it was a thick jelly; if the result of successive stimulations were allowed to collect in the cannula, the whole came out in a cohering clot. From time to time, indeed, it was found necessary to clear the duct by squeezing the clotted saliva out of it. The saliva was kindly analysed for me by Mr. Waters, of Christ's College. It contained 8·477 per cent. of solids, of which 7·803 per cent. was organic matter.

It is possible, though I think very improbable, that in stimulating the sympathetic, Jacobson's nerve was stimulated reflexly, and hence the apparent presence of secretory fibres in the sympathetic. The highest percentage of solids obtained by Heidenhain* by combined stimulation of the sympathetic and Jacobson's nerve was 2·82 per cent.

The gland examined fresh showed a beginning clear zone, often only seen as a thinning of granules at the periphery; after a larger amount of saliva had subsequently been obtained by pilocarpin injection the clear zone became more marked.

The Sub-maxillary Gland of the Rabbit.

In the fresh state this has one very characteristic point, the transition cells are crowded with large conspicuous granules. The outlines of the ductule or transition cells apart from these granules are quite unrecognisable, they appear as patches and bands much darker than the surrounding gland substance. Some of the cells, which from their shape would be called ductule cells, certainly contain similar granules, though they are, I am inclined to believe, absent from the ductule cells, springing immediately from the ducts. The absence of cell outlines and the difficulty of obtaining thin sections of the fresh gland makes a decision on this point difficult to arrive at.

The alveoli are clearer than in the parotid; they contain granules not so refractive and rather smaller than those in the parotid. I have been unable in the animals of varying age with which I have worked to find any *constant* difference in hunger and digestion, exceptions to what I regard as the rule being all too frequent. The rule is, I think, mainly from experiments on stimulation of the sympathetic and injection of pilocarpin, that during secretion the granules disappear from the outer portion of the cells. The exceptions I have spoken of

* *Op. cit.*, p. 30.

were the following:—In some adult rabbits in digestion the clear zone was very doubtfully apparent; in some young rabbits in hunger there were distinctly two zones, and in one or two of adult and young sympathetic stimulation produced but very little effect.

Nussbaum * found that the transition cells stained black with osmic acid, whilst the alveolar cells were but lightly coloured. His conclusion from this that the transition cells formed the ferment was contested by me † on the ground that if the gland were treated with alcohol before treating with osmic acid, the transition cells did not stain black, and that it was highly improbable that the ferment had been dissolved by alcohol. Nussbaum, in a later paper,‡ upholds his opinion and rejects my argument on the ground that he can obtain a substance from the gland which is insoluble in alcohol, and which yet does stain black with osmic acid, this substance being the ferment.

It is a matter easy to decide; a fresh gland is placed in absolute alcohol for twenty-four hours; sections are made and placed in osmic acid one per cent. for the time mentioned by Nussbaum, viz., two hours, and examined. The whole has a yellowish-brown tint, the transition cells are but a trifle more deeply stained than the alveolar cells; they have to be looked for instead of striking the eye in the manner they do when the gland is treated with osmic acid first and alcohol afterwards. A stay of twenty-fours in the acid does not materially alter the appearances. It is, perhaps, hardly necessary to bring forward my original argument; for numerous later experiments have shown me that the prominence of the transition cells and their depth of tint depend on the subsequent treatment with alcohol rather than on the prior treatment with osmic acid. The gland, placed in osmic acid one per cent. and that only, for two hours, has the ducts darkest stained, the transition cells are slightly deeper in tint than the alveolar cells, mainly because the former are evenly stained throughout, whereas the latter are not; both are yellowish-brown. One, two, or even more days do not make the transition cells conspicuous. But if, after two hours in osmic acid, the glands are placed in dilute alcohol 50 to 75 per cent. for twenty-four hours, sections show the brown-black clumps of the transition cells scattered amongst the much lighter-coloured alveoli. The ducts are in this case equally or even less dark than the transition cells. If alcohol be used absolute instead of diluted, the appearances are similar, but in the first day or two at any rate, less marked.

In other glands, too, I have found that the selective staining of osmic acid is best brought out by subsequent treatment with alcohol.

The resting and active glands treated with osmic acid and alcohol

* Max Schültze. "Arch. f. Mik. Anat.," Bd. xiii, p. 724, 1877.

† Foster. "Journal of Physiology," vol. i, p. 68, 1878.

‡ Max Schültze. "Arch. f. Mik. Anat.," Bd. xvi, p. 543, 1879.

show differences similar to those described above for the parotid under similar circumstances. In early digestive stages, however, the granules appear sharper and more refractive, the distinctness of the cell network depends largely on the alcohol treatment.

I can confirm the observations of Bermann* on the presence of a "tubular gland" in the sub-maxillary of the rabbit. The large lumen and small cubical epithelium of the majority of the tubes do not suggest that it has any important secretory function.

The Infraorbital and Lachrymal Glands of the Rabbit.

In these glands the two zones in the secretory condition are even more conspicuous than in the parotid. Even more readily too than in parotid can lobules thin enough for observation be obtained.

In different animals the alveoli may be granular throughout, or may have a more or less distinct outer non-granular zone. But whichever condition occurs the granules can always be diminished by setting up a secretion with pilocarpin. A similar change, I think, too, occurs on stimulation of the sympathetic.

These glands are distinguished from the parotid and sub-maxillary by preserving fairly their normal aspect after osmic acid and alcohol treatment. The resting gland is not unlike, in chief features, the resting parotid; the network is particularly obvious.

When a clear zone is present in the fresh state it stains easily with osmic acid, showing ordinarily no network; with a small non-granular zone the granular zone appears as a network in the preserved specimen, as the former increases the latter appears more granular and less as a network. In activity the nuclei become spherical, move towards the middle of the cells, and become less conspicuous.

Recalling what has been said of the parotid and sub-maxillary glands, it will be seen that in these, as well as in the infraorbital and lachrymal glands, the alveolar cells in the secreting condition stain much more homogeneously with osmic acid than they do in the resting condition. In the two latter glands the homogeneous staining corresponds in extent with the clear zone of the fresh specimen, in the parotid this is to a slight extent only the case, in the sub-maxillary gland scarcely at all. I am inclined to think that by a modified use of osmic acid, the normal appearances in all these glands may be more nearly preserved than is commonly the case, though, undoubtedly, a great part of the divergence mentioned above in their behaviour in the secretory state, depends on a chemical difference in the substances broken down and re-formed.

The investigations, of which the foregoing is a brief account, were carried on in the Physiological Laboratory of the University of Cambridge.

* "Ueber die Zusammensetzung der Glandula Submaxillaris." Würzburg, 1878.

II. "On the Changes in Pepsin-forming Glands during Secretion." By J. N. LANGLEY, M.A., Fellow of Trinity College, Cambridge, and H. SEWALL, B.Sc., Fellow of the Johns Hopkins University, Baltimore, U.S.A. Communicated by Professor MICHAEL FOSTER, M.D., F.R.S. Received October 13, 1879.

The Œsophageal Glands of the Frog. (Rana temporaria.)

In a frog three to four days after food the acini of the œsophageal glands in the living state are granular throughout, and so thickly granular that the outlines of the alveoli are but faintly indicated; the outlines of the cells not at all seen, nor are there any indications of nuclei.

Shortly after food is given, the granules thin away at the peripheries of the alveoli and so render the alveolar outlines more obvious. This thinning proceeds so rapidly that in a few hours there is a well-marked clear zone in the outer part of each alveolus, the part nearest the basement membrane.

Later the clear zone becomes larger, the granular zone becoming smaller, but as the clear zone enlarges it ceases to form in section a ring, it dips down into the granular zone at intervals, or in other words, the cells in using up their stored granules do not all use them up at the same rate, some are more energetic than others, so that the granular zone takes in section a somewhat star shape. In addition to this, the star-form of the granular zone is aided by the granules along the margins being less readily used up than those in the interior of the cells.

Nuclei are as little seen in the digesting as in the resting gland. The glands having then as digestion goes on got rid of a large number of their granules, proceed to form others as digestion ceases. The clear spaces between the points of the star are filled up, and the clear zone forms granules from within outwards, until the granules once more extend to the basement membrane.

An indication of the changes taking place in the alveoli of the glands can be seen in the œsophagus itself with the unaided eye; in a hungry frog the œsophagus is an opaque white, in a digesting frog it is greyer and more transparent.

Frogs in an ill state of health do not show the full granular condition characteristic of the hungry frog, the granules are much less numerous, and often, indeed, do not extend to the basement membrane; with these, however, as with healthy frogs, the granules diminish during digestion.

The granules we have hitherto spoken of may be called "central"

granules, to distinguish them from others which occur immediately beneath the basement membrane. These "border" granules are smaller and more highly refractive; they occur usually in small clumps.

The points mentioned above as observable in the fresh tissue, can also in the main be observed in glands treated first with osmic acid and then with alcohol. There are one or two things to be noticed in sections of glands so treated. The border granules stain more deeply and readily than the central granules. The mucous cell granules have disappeared, the cells present the usual characters of a mucous cell in hardened specimens. In the resting gland the nuclei are generally hidden; in the digesting gland the nuclei are large, spherical, situated in the clear zone, with faint outlines; the nucleolus is deeply stained and prominent. In the digesting there are fewer obvious mucous cells than in the resting gland.

Nussbaum* has made some observations on the glands treated with osmic acid; it will be noticed that we differ from him in almost every respect.

Swieçicki† found the glands after treatment with alcohol, to be, in the digesting state as a rule, larger, more granular, and better staining. The greater granulation here obviously only means that in the digesting state there is a greater amount of substance precipitable by alcohol in the granular condition. The cells when alive are less granular during digestion.

Partsch,‡ working also with hardened specimens, found the glands to increase in size as they secreted, but only for the first 5—10 hours.

Whilst not prepared to deny that an increase of size may in the first stages take place, we have failed to observe it, and are inclined to think that as the granules are used up, there is a steady diminution in the size of the glands.

Comparing Grützner's§ observations on the amount of ferment present in the glands in hunger and digestion with our own on their varying granular state, we find that the ferment diminishes during digestion as do the granules. For this and other reasons we are inclined to consider the granules as stored up cell products which, on suffering molecular re-arrangement, give rise amongst other substances to the proteid ferment.

Nussbaum also holds this, ascribing however both to granules and ferment a different maximal time. We cannot agree with Nussbaum's view that the depth of staining with osmic acid tells the amount of ferment unless perhaps it is restricted much more than he is inclined

* Max Schültze. "Arch. f. Mik. Anat." Bd. xiii, p. 746, 1877.
† Pflüger. "Archiv f. d. Ges. Physiologie," Bd. xiii, p. 444, 1876.
‡ Max Schültze. "Arch. f. Mik. Anat." Bd. xiv, p. 179, 1877.
§ Pflüger. "Arch. f. d. Ges. Physiologie," Bd. xvi, p. 122, 1877.

to restrict it. On his view it appears to us the border rather than the central granules should be connected with the ferment.

The Gastric Glands of the Newt (Triton Tæniatus).

In the newt the glands were observed through the muscular coat of the stomach, with a rapid capillary circulation still going on.

Twenty-four hours after feeding, the glands of the fundus are thickly granular throughout; about three hours after feeding, the maximal change takes place; it corresponds in the main to that already described for the œsophageal glands of the frog. In the newt the lumina and cell outlines become more conspicuous, the clear zone is generally less sharply marked off from the granular zone; and the star arrangement of the granules is apparently only caused by the presence of granules along the lateral margin of the cells after those in the interior have disappeared.

The glands recover their granular appearance comparatively quickly; in six hours after feeding, the granules have usually again crept up to the periphery, they then increase in number throughout the cells up to about the twenty-fourth hour. Later than this they diminish somewhat, in six days the peripheries of the glands have become more sparsely populated. It is at this period that digestion changes are most readily observed.

The pyloric glands are clear, or very finely granular, changes in them would be much less conspicuous, in such observations as we have made; we have seen no difference in the hungry and digesting states.

In *Triton cristatus* the digestive changes are of the same nature, but much less pronounced.

The Gastric Glands of Stickleback (Gasteropodus trispinatus ?)

We chose the stickleback mainly for its voracity, there is seldom any difficulty in feeding it.

In the gastric glands of the hungry fish, the granules thin away somewhat from the centre to the periphery, the lumina are inconspicuous; three to five hours after feeding the lumina are much larger, the granules are aggregated about it leaving a peripheral clear rim, the glands are more unequal in size, some having lost more granules and diminished more in size than others.

The Gastric Glands of Mammals.

Examining in the fresh state, the glands of the fundus of the dog, cat, or rat, we find that the body of each gland is marked out sharply into two parts. A central part near the lnmen, consisting of many rather large sharp granules, these granules form a central mass, which here

and there branches out to the periphery, no cell outline at all can be seen. A peripheral very finely granular almost homogeneous part.

The necks of the glands are finely granular, with scattered small dark granular patches. By treatment with 33 per cent. potash and other re-agents, it can be seen that the dark granules are contained in the chief-cells; the outer finely granular or clear portion of the glands being the border-cells. At the outer border, the outlines of the border-cells can be well seen; at the junctions of the cells, however, the outlines disappear. No nuclei are visible. The small dark granular patches in the necks of the glands are found to be also the chief-cells which are here and there scattered amongst the border-cells.

The pyloric glands in the fresh state are fairly transparent, finely granular to homogeneous. The lumina are usually distinct, and often stretch up between the cells; apart from this no cell outlines are visible. No nuclei are to be seen.

Heidenhain* described correctly the appearance of the fresh fundus glands; except that he mentions the nuclei of the border-cells as being normally just visible.

Ebstein,† I think, implies that the pyloric glands are normally not coarsely granular.

Yet Heidenhain, Ebstein, Grützner, and others, who advocate the identity of the chief-cells with the pyloric gland cells, do not seem to have noticed the difference existing between the two cells in the fresh state. The difference is, however, most marked.

The above description is of the chief-cells in the resting state. If the glands are observed a few hours after a hungry animal has been fed, the central granular mass in the body of the gland is found to have very much diminished. A transverse section instead of showing a thick mass of granules stretching here nearly, there quite to the periphery, show a narrow rim or star of granules.

During secretion then the granules of the chief-cells are used up.

The stomach of the rabbit deserves especial consideration. It may be divided into four portions, each in succession graduating into the next.

1. The *Fundus* or dilatation at the cardiac end of the stomach. This is to the eye of an opaque white colour. The body of each gland shows comparatively few border-cells, but consists mainly of a darkly granular mass of chief-cells.

2. The *Greater Curvature*. This is to the eye somewhat pink; in fresh specimens scarcely any dark granules are to be seen. In passing from the fundus to the greater curvature, the number of dark granules gradually diminishes, at first being confined more and more to the

* Max Schültze. "Arch. f. Mik. Anat." Bd. vi, p. 368, 1870.

† P. 515, 1870.

deeper parts of the glands than those also taking a thin rim or star arrangement.

3. The *Lesser Curvature.* The glands here have the character given above for pyloric glands.

4. The *Pylorus.* The glands are simply shallow depressions of the mucous membrane, and are still more transparent than the glands of the smaller curvature.

In hunger the opaque thickly granular region encroaches on the greater curvature.

In digestion the granules diminish, beginning at the opaque part near the greater curvature, so that the opaque thickly granular region is more and more limited to the extremity of the fundus.

As far as concerns the diminution of the coarse granules of the chief-cells during digestion, this is but a repetition of what we have already said of the dog, cat, and rat. The point of additional interest is this. Sections of alcohol-hardened fragments of the greater curvature show that here, as well as in the fundus, there are a large number of chief-cells. Probably no one examining such sections of the greater curvature would consider them as other than typical fundus glands with chief-cells and border-cells. Yet the former cells in the fresh state do not contain the coarse granules characteristic of chief-cells.

Deferring for a moment the consideration of this, we would mention some experiments we have made on the amount of ferment-content of the various parts of the rabbit's stomach.

Equal-sized pieces of the mucous membrane of a rabbit were taken and put in absolute alcohol. A. From the fundus. B. From the greater curvature. C. From the smaller curvature, including part of the pylorus. These were kindly tested for ferment by Mr. Sheridan Lea, of Trinity College, both by the ordinary digestive method and by Grützner's colorimetric method. The result was that A contained more ferment than either B or C; B and C contained a not very different amount, B rather more than C.

Now A contained border-cells and coarsely granular chief-cells; B contained more border-cells than A, but chief-cells with scarcely any coarse granules; C contained only pyloric cells, with possibly here and there a solitary border-cell.

Hence the part with the pre-eminently large number of border-cells offered hardly appreciably more pepsin than the part containing few, if any; the conclusion that the border-cells do not form pepsin appears to us unavoidable.

Moreover a comparatively very large amount of pepsin was contained in the part of the mucous membrane with a densely granular chief-cell. This, we think, is enough to connect the ferment with these granules; in this opinion we are confirmed by the result of the following experiment. Two rabbits of the same breed, and in all

respects as much as possible alike, were taken; one was killed hungry, *i.e.*, twenty-four hours after feeding, the other in full digestion some 5—6 hours after a plenteous meal. The gastric mucous membrane showed the usual difference to the unaided eye and to microscopical examination; equal-sized pieces were removed from as near as possible a corresponding position of the fundus of each animal. They were tested by Mr. Lea for ferment, the resting more granular piece was found to possess a not inconsiderably greater amount of ferment than the active less granular piece. We may recall that Grützner found the amount of ferment to diminish during digestion.

There is one other fact which leads us to connect the chief-cell granules with the ferment; in protracted hunger the chief-cell granules, instead of increasing diminish, as we have mentioned above, occurs in the newt; according to Grützner, in protracted hunger, the ferment-content of the gastric mucous membrane diminishes.

To return to the consideration of the meaning of the finely granular chief-cells of the greater curvature. These differ but little, histologically, from the pyloric gland cells. Both form pepsin. Such close similarity in form and function justify us in considering them to be fundamentally the same kind of cell. How, then, do they stand related to the coarsely granular chief-cells? The distinguishing features of the latter are their coarsely granular appearance and their large ferment-content; in an extreme digestive stage these features become much less conspicuous. We are led, then, to think that the coarsely granular chief-cells are but a more highly developed form of the cells found in the other parts of the stomach as the finely granular chief-cells and pyloric gland cells.

The greater curvature of the rabbit probably corresponds to the intermediary portion of the stomachs of dog, cat, and rat.

It will be noticed that in the main we advocate Heidenhain's views, objecting, however, to the statement that the chief-cells and the pyloric gland cells are *identical*, since their normal appearance is so strikingly different.

The investigations, of which the foregoing is a brief account, were carried on in the Physiological Laboratory, Cambridge.

III. "Report on Phyto-Palæontological Investigations of the Fossil Flora of Sheppey." By Dr. Constantin Baron Ettingshausen, Professor in the University of Graz, Austria. Communicated by Professor Huxley, Sec. R.S. Received November 3, 1879.

One of the most important, if not the most important, locality for the Eocene Flora of Great Britain, and perhaps of the tertiary forma-

tion generally, is the London Clay of the Isle of Sheppey, in which are found great numbers of plant remains belonging to many different kinds of fossil fruits and seeds. After an examination of the rich collection in the British Museum, I feel now sure that we possess, in the fruits and seeds of Sheppey, the key to a more precise determination of many of the genera and species of fossil plants which in other localities are known only by their leaves.

The literature of the Sheppey fruits is not very extensive; a detailed account of all the works relating to it is published in Mr. Gardner's "Introduction to our Monograph on the British Eocene Flora." Palæontographical Society, 1879, p. 11. The only work on this subject with scientific determinations, and which need here be referred to, was published in the year 1840 by James Scott Bowerbank, and is entitled "A History of the Fossil Fruits and Seeds of the London Clay." He enumerates twelve genera, which are divided by him into nine families. The genera are as follows: Nipadites, Hightea, Petrophiloides, Cupressinites, Cupanoides, Tricarpellites, Wetherellia, Cucumites, Faboidea, Leguminosites, Mimosites, Xylinosprionites. Of these only one (Nipadites), belongs to the Monocotyledons and one (Cupressinites) to the Gymnospermæ, while the rest are Dicotyledons.

I am now able materially to advance the knowledge of this Flora. Since my investigations in the course of the winter 1878–9, at the British Museum, I have ascertained that the Fossil Flora of Sheppey contains, including those above mentioned, at least 72 genera and 200 species, which may be distributed into 41 families. Of these genera one belongs to the Thallophyta, 7 to the Gymnospermæ, 18 to Monocotyledons, 43 to the Dicotyledons, and 3 are indeterminable.

The existence of this Flora and generally of the Eocene Flora of Great Britain required, we believe, at least, a sub-tropical climate. This is indicated by many of the ferns and palms, and by the Musaceæ, Pandanæ, Cinchonaceæ, Loganiaceæ, Sapotaceæ, Ebenaceæ, Büttneriaceæ, Sapindaceæ, &c.

Some only of the fossil fruits and seeds of Sheppey can be placed in living genera, but with regard to the rest, forming a considerable part of these, it was impossible, notwithstanding a careful comparison. I therefore assume that some of the fruits and seeds belong to genera which no longer exist in the present Flora of the world. In several of these extinct genera, however, I recognise their affinity with living genera, or at least determine the family to which they belong. I have expressed this in the name of the genus. But with many even that was impossible, and these I have placed in the meantime under the provisional name Carpolithes. It is an important fact that the number of such extinct forms is relatively much larger than it is in any of the already known Miocene Floras. I have also discovered fruits, but chiefly leaves belonging to many of the genera of the

Sheppey Flora in the Fossil Floras of Bournemouth and Alum Bay in the collections of the British Museum, and that of Mr. John Starkie Gardner, indicating that in age these are not far removed. It is well known that the Sheppey Flora preceded the other two. The genera which, I feel sure, are common to Sheppey and Bournemouth are: Sphæria, Sequoia, Cyperites, Smilax, Sabal, Iriartea, Aronium, Quercus, Juglans, Liquidambar, Proteoides, Laurus, Nyssa, Cinchonidium, Apocynophyllum, Sapotacites, Diospyros, Magnolia, Acer, Sapindus, Cupania, Eugenia, Eucalyptus, Metrosideros, Bauhinia.

I believe that even some species of these genera are the same in both Floras. It is surely probable that the fruits and seeds of Sheppey were related to the leaves found at Bournemouth and Alum Bay; and it would be, therefore, undesirable always to propose separate specific names for the related fossils found in these different localities.

Among the plant-fossils of Bournemouth and Alum Bay I also found many leaves which I could not class with existing genera. There is probability that these partially correspond with the extinct fruit- and seed-genera of Sheppey.

Before I enumerate the genera and species of the Fossil Flora of Sheppey, I have to remark as follows:—

Amongst the Sheppey fossils are now and then found fragments of the basis of the leaf of a palm, probably of Sabal major. On such a fragment I found the apothecia of a Sphæria. Of the Gymnospermæ of Sheppey there were found fruits and seeds of the Sequoia Bowerbankii, also fragments of twigs. The seeds of the Cupressineæ and Abietineæ had lost their wing-like expansions, which shows that the fruits and seeds of Sheppey were carried some distance in water, consequently their delicate membranous wings were injured and broken off by rubbing. There are therefore no perfect winged fruits and seeds to be found. In fact, even the firmer wings of the Acer-fruit have been entirely lost, and it is impossible to determine the species of the Acer-nucules, which remain.

The appearance of the Salisburia seeds is interesting. They are very remarkable for their sharp, prominent edge. The easily-determinable leaves of this genus have not as yet been found in the Eocene Flora of Great Britain.

The Agave is indicated by a valve of fruit; Smilax, of which leaves are not unfrequently found at Bournemouth, is indicated by a berry. Of Musa, of which only leaves had as yet been found, there are seeds. Of Amomum, two kinds of fruit have been found. These have hitherto been mistaken for smaller fruits of the Nipadites. Of particular interest are the many species of palms. The fruits and seeds of some, for instance, Sabal major, Trinax Bowerbankii, Elæis eocenica, Iriartea striata, Livistona eocenica, have been found. Of the Sabal and Iriartea the leaves are found at Bournemouth. The Elæis

eocenica, the most common palm of the Sheppey Flora, is nearly allied to the E. melanococca, and the Livistona eocenica to the L. chinensis.

I do not yet know whether the Aroidea seed, which I have placed in Aronium, might be united with the Aroidea leaf of Bournemouth. On the other hand, I think it is very likely that some of the kinds of oak fruits correspond with some of the kinds of oak leaves, which are to be met with at Bournemouth. Two of the Bournemouth species are also found in the Miocene Flora, and one of these, Quercus Lonchitis, also in other Eocene Floras.

A small nut shows all the characters of the Corylus, which is found in the Miocene Flora. The absence of the Fagus is very remarkable, as two kinds of leaves, which can only belong to this genus, have been found in Bournemouth. Fagus is frequently found in the Miocene and Post-Tertiary formations, and also in the Cretaceous formation, and I believe, therefore, that it may still be found in Sheppey.

The Liquidambar-fruit of Sheppey may belong to the same species as the inflorescence of Liquidambar, which I found amongst the fossils of Bournemouth. The Laurus-berry, which I have found amongst the Sheppey fruits, is placed by me in Laurus Lalages, the leaves of which have been not only found in Bournemouth, but also in the Austrian Eocene (Sotzka, Hæring), where they occur associated with berries.

The occurrence of a species of Nyssa, I think, may be also accepted for the Bournemouth strata. Of the Proteaceæ, besides Petrophiloides, a seed belonging to the Proteæ occurs, perhaps corresponding with the leaf which I have seen among the fossils of Alum Bay. In Bournemouth and Alum Bay were found the seeds of some other Proteaceæ which are not in Sheppey, as they have delicate wings. Some of the cones referred by Bowerbank to Petrophiloides belong to Sequoia.

The Gamopetalæ are represented by many genera, of which almost all appear also in the Miocene Flora. The fruit of Cinchonidium of Sheppey and the leaves of a species of Cinchonidium from Bournemouth may belong together. I accept the same for the Apocynophyllum fruit of Sheppey and the corresponding leaf of Bournemouth. This last accords in all its characters with A. Reussi, which also appears in the fossil Flora of Sagor. But I have not found up to the present time, in the Eocene Flora of Great Britain, any leaf belonging to the characteristic genus Strychnos, the seeds of which are met with at Sheppey.

Of the Sapotaceæ there are two species of seeds, which perhaps correspond with two species of Sapotaceæ leaves of the Bournemouth Flora. One of these extends throughout in the Tertiary Flora; the other, on the contrary, seems to be peculiar to the Eocene Flora. Also peculiar to this Flora are two species of Diospyros, which are common both to Sheppey and Bournemouth.

The species of Symplocos is common to Sheppey and Sagor. In these two localities the putamen of this species were found. In corresponding abundance are represented the Dialypetalæ. They are specially characteristic of the Eocene Flora in general, and of the Sheppey Flora in particular. To the first belong Magnolia eocenica, Eugenia eocenica, Sapindus eocenicus, Metrosideros microcarpa, and Bauhinia primigenia, whose leaves or fruits are to be found associated together in Bournemouth. To the last belong the genera Menispermacites, Victoria, Thlaspidium, Corchorites, Theobroma, Lawsonia; the species Illicium Apollinis, Nelumbium microcarpum, Cucumites Sheppyensis, Cotoneaster Sheppyensis, Prunus prisca and Druidum, Amygdalus eocenica, and Sporadum, Podogonium Sheppyense, &c. Belonging also to other Eocene Floras and to the Miocene Flora we have here only the Dialypetalous plants, Nelumbium Buchii, and Eucalyptus oceanica.

Amongst the fruits and seeds of Sheppey we find also some species of herbaceous or tender plants whose leaves would not be preserved in the Tertiary strata. To these belong the seeds of Solanites elegans, Menispermites abutoides, Cucumites Sheppyensis, the fruits of Thlaspidium ovatum, and of the Corchorites species.

In conclusion I desire to express my thanks to the Royal Society for the important assistance it has rendered me by the grants from its funds by which I have been enabled to prosecute my investigations in England; and for personal assistance in this work I desire to record my indebtedness to Sir Joseph Hooker, Dr. Henry Woodward, Dr. William Carruthers, and Mr. John Starkie Gardner.

The descriptions and illustrations of the species, here enumerated, will appear in the Monograph now in course of publication by the Palæontographical Society.

Genera and Species of the Fossil Fruits and Seeds of Sheppey.

Thallophyta.

Sphæria Flabellariæ. *Ett. and Gard.*

Gymnospermæ.

Cupressineæ.

Callitris curta. *Bowerb. sp.*
(Syn. Cupressinites. *c. Bowerb.*)
Callitris comptoni. *Bowerb. sp.*
(Syn. Cupressinites. *c. Bowerb.*)
Solenostrobus subangulatus. *Bowerb. sp.*
(Syn. Cupressinites. *s. Bowerb.*)
Solenostrobus corrugatus. *Bowerb. sp.*
(Syn. Cupressinites. *c. Bowerb.*)
Solenostrobus sulcatus. *Bowerb. sp.*
(Syn. Cupressinites. *s. Bowerb.*)
Solenostrobus semiplotus. *Bowerb. sp.*
(Syn. Cupressinites. *s. Bowerb.*)
Hybothya crassa. *Bowerb. sp.*
(Syn. Cupressinites. *c. Bowerb.*)
Cupressinites globosus. *Bowerb.*
„ elongatus. „
„ recurvatus „
„ subfusiformis. „

Abietineæ.

Sequoia Bowerbankii. *Ett.* and *Gard.*
(Syn. Petrophiloides Richardsoni. *Bowerb.*
„ Petrophiloides cylindricus. *Bowerb.*)
(Syn. Petrophiloides cellularis. *Bow.*
„ „ ellipticus. „)
Pinus Sheppyensis. *Ett. and Gard.*

Taxineæ.

Salisburia eocenica. *Ett. and Gard.*

MONOCOTYLEDONES.

Cyperaceæ.

Cyperites eocenicus. *Ett. and Gard.*

Liliaceæ.

Agave eocenica. *Ett. and Gard.*

Smilaceæ.

Smilax pristina. *Ett. and Gard.*

Najadeæ.

Caulinites Sheppyensis. *Ett. and Gard.*

Musaceæ.

Musa eocenica. *Ett. and Gard.*

Zingiberaceæ.

Amomum Sheppyense. *Ett. and Gard.* Amomum stenocarpum. *Ett. and Gard.*

Pandaneæ.

Nipa Burtini. *Brongn. sp.*
(Syn. Nipadites umbonatus. *Bowerb.*
„ crassus. „
„ cordiformis. „
„ pruniformis. „
„ acutus. „
„ clavatus. „
„ giganteus. „)
Nipa elliptica. *Bowerb. sp.*
(Syn. Nipadites. *e. Bowerb.*)
Nipa lanceolata. *Bowerb. sp.*
(Syn. Nipadites. *l. Bowerb.*)
Nipa Parkinsonis. *Brongn. sp.*
(Syn. Nipadites P. *Bowerb.*
„ turgidus. *Bowerb.*
Nipa semiteres. *Bowerb. sp.*
(Syn. Nipadites. *s. Bowerb.*
„ pyramidalis. *Bowerb.*)

Palmæ.

Oenocarpus Sheppyensis. *Ett. and Gard.*
Areca prisca. *Ett. and Gard.*
„ recentior. *Ett. and Gard.*
Iriartea striata. *Ett. and Gard.*
„ punctata. *Ett. and Gard.*
Livistona eocenica. *Ett. and Gard.*
Sabal major. *Ung.*
„ Dryadum. *Ett. and Gard.*
„ Oreadum. *Ett. and Gard.*
Sabal Dianæ. *Ett. and Gard.*
Chamærops borealis. *Ett. and Gard.*
Trinax Bowerbankii. *Ett. and Gard.*
var. *α.* globosa.
var. *β.* elliptica.
Bactris prisca. *Ett. and Gard.*
Asterocaryum Europæum. *Ett. and Gard.*
Elæis eocenica. *Ett. and Gard.*

Aroideæ.

Aronium eocenicum. *Ett. and Gard.*

DICOTYLEDONES.

APETALÆ.

Cupuliferæ.

Quercus Lonchitis. *Ung.*
„ drymeja. *Ung.*
Quercus eocenica. *Ett. and Gard.*
Corylus primigenia. *Ett. and Gard.*

Juglandeæ.

Juglans eocenica. *Ett. and Gard.*

Euphorbiaceæ.

Euphorbiophyllum eocenicum. *Ett. and Gard.*

Balsamifluæ.

Liquidambar eocenicum. *Ett. and Gard.*

Proteaceæ.

Petrophiloides imbricatus. *Bowerb.*
„ conoideus. „
Petrophiloides oviformis. *Bowerb.*
Proteoides bisulcatus. *Ett. and Gard.*

Laurineæ.

Laurus Lalages. *Ung.*

Nyctagineæ.

Nyssa eocenica. *Ett. and Gard.*

GAMOPETALÆ.

Cinchonaceæ.

Cinchonidium priscum. *Ett. and Gard.*

Loganiaceæ.

Strychnos Urani. *Ett. and Gard.*

Apocynaceæ.

Apocynophyllum Sheppyense. *Ett. and Gard.*

Solanaceæ.

Solanites elegans. *Ett. and Gard.*

Sapotaceæ.

Sapotacites Mimusops. *Ett.*
Sapotacites chrysophylloides. *Ett. and Gard.*

Ebenaceæ.

Diospyros eocenica. *Ett. and Gard.*
Diospyros Pleadum. *Ett. and Gard.*

Symploceæ.

Symplocos radobojana. *Ung.*

DIALYPETALÆ.

Menispermaceæ.

Menispermacites abutoides. *Ett.*

Magnoliaceæ.

Magnolia eocenica. *Ett. and Gard.*
Illicium Apollinis. *Ett. and Gard.*

Cruciferæ.

Thlaspidium ovatum. *Ett. and Gard.*

Nymphaeaceæ.

Nelumbium Buchii. *Ett.*
„ microcarpum. *Ett. and Gard.*
Victoria Sheppyensis. *Ett. and Gard.*
„ Najadum. *Ett. and Gard.*

Cucurbitaceæ.

Cucumites Sheppyensis. *Ett. and Gard.*

Büttneriaceæ.

Theobroma Nimrodis. *Ett. and Gard.*
Theobroma Zoroastri. *Ett. and Gard.*

Malvaceæ.

Hightea elliptica. *Bowerb.*
(Syn. Hightea attenuata. *Bowerb.*
„ fusiformis. „)
Hightea elegans. *Bowerb.*
„ inflata. „
Hightea oviformis. *Bowerb.*
„ turbinata. „
„ orbicularis. „
„ minima. „
„ turgida. „

Tiliaceæ.

Apeibopsis variabilis. *Bowerb. sp.*
(Syn. Cucumites. *v. Bowerb.*)
Corchorites quadricostatus. *Ett. and Gard.*
„ quinquecostatus. *Ett. and Gard.*

Acerineæ.

Acer sp. adhuc indeterminata.

Sapindaceæ.

Sapindus eocenicus. *Ett. and Gard.*
Cupania lobata. *Bowerb. sp.*
(Syn. Cupanoides. *l. Bowerb.*)
Cupania subangulata. *Bowerb. sp.*
(Syn. Cupanoides. *s. Bowerb.*)
Cupania tumida. *Bowerb. sp.*
(Syn. Cupanoides. *t. Bowerb.*)
Cupania depressa. *Bowerb. sp.*
(Syn. Cupanoides. *d. Bowerb.*)
Cupania corrugata. *Bowerb. sp.*
(Syn. Cupanoides. *c. Bowerb.*)
Cupania grandis. *Bowerb. sp.*
(Syn. Cupanoides. *g. Bowerb.*)
Cupania inflata. *Bowerb. sp.*
(Syn. Cupanoides. *i. Bowerb.*)
Cupania pygmæa. *Bowerb. sp.*
(Syn. Cupanoides. *p. Bowerb.*)

Myrtaceæ.

Eugenia eocenica. *Ett. and Gard.*
Eucalyptus oceanica. *Ung.*
Metrosideros microcarpa. *Ett. and Gard.*

Lythrarieæ.

Lawsonia Europæa. *Ett. and Gard.*

Pomaceæ.

Cotoneaster Sheppyensis. *Ett. and Gard.*

Amygdaleæ.

Prunus prisca. *Ett. and Gard.*
„ Druidum. *Ett. and Gard.*
Amygdalus eocenica. *Ett. and Gard.*
„ Sporadum. „ „

Papilionaceæ.

Podogonium Sheppyense. *Ett. and Gard.*
Bauhinia primigenia. „ „
Faboidea longiuscula. *Bowerb.*
„ crassa. „
„ crassicutis. „
„ planidorsa. „
„ symmetrica. „
„ plana. „
„ marginata. „
„ semicurvilinearis. „
„ larga. „
„ complanata. „
„ subdisca. „
„ oblonga. „
„ ovata. „
„ ventricosa. „
„ robusta. „
„ pinguis. „
„ subrobusta. „
„ planimeta. „
„ quadrapes. „
„ bifalcis. „
„ tenuis. „
„ subtenuis. „
„ rostrata. „
Faboidea doliformis. *Bowerb.*
„ acuta. „
„ angustissima. *Ett. and Gard.*
Leguminosites subovatus. *Bowerb.*
„ crassus. „
„ elegans. „
„ rotundatus. „
„ longissimus. „
„ gracilis. „
„ enormis. „
„ dimidiatus. „
„ lentiformis. „
„ planus. „
„ lobatus. „
„ inconstans. „
„ reniformis. „
„ curtus. „
„ subquadrangularis. „
„ æquilateralis. „
„ trapeziformis. „
„ cordatus. „
Xulinosprionites latus. „
„ zingiberiformis. „
Mimosites Brownianus. „

PLANTÆ INCERTÆ SEDIS.

Wetherellia variabilis. *Bowerb.*
Tricarpellites communis. „
„ patens. „
„ curtus. „
„ crassus. „
„ gracilis. „
„ aciculatus. „
„ rugosus. „
Carpolithes zizyphoides. *Ett. & Gar.*
„ hydrophylloides. „ „
„ metrosideroides. „ „
„ colletioides. „ „
„ Nimrodis. „ „
„ populoides. „ „
„ bispermus. „ „
„ amygdaloides. „ „
„ caryopsiformis. „ „
„ tenue-punctatus. „ „
„ radiato-punctatus. „ „
„ verrucosus. „ „
„ Morrisii. „ „
„ Zoroastri. „ „
„ bruceoides. „ „
Carpolithes reticulato-rugosus. *Ett. & Gar.*
„ affinis. „ „
„ Atlantidis. „ „
„ nyssæformis. „ „
„ Napæarum. „ „
„ folliculiformis. „ „
„ brevicristatus. „ „
„ cruciferinus. „ „
„ franguloides. „ „
„ websterioides. „ „
„ disciformis. „ „
„ musæformis. „ „
„ punctato-striatus. „ „
„ sulcatus. „ „
„ costatus. „ „
„ caryotoides. „ „
„ biocculatus. „ „
„ plurioculatus. „ „
„ lineatus. „ „
„ breviangulatus. „ „
„ circumscriptus. „ „
„ subalatus. „ „

IV. "A Memoir on the Single and Double Theta-Functions." By A. Cayley, F.R.S., Sadlerian Professor of Pure Mathematics in the University of Cambridge. Received November 14, 1879.

(Abstract.)

The theta-functions, although arising historically from the elliptic functions, may be considered as in order of simplicity preceding these, and connecting themselves directly with the exponential function (e^x or) exp x: viz., they may be defined each of them as a sum of a series of exponentials, singly infinite in the case of the single functions, doubly infinite in the case of the double functions, and so on. The number of the single functions is=4; and the quotients of these, or say three of the functions, each divided by the fourth, are the elliptic functions, sn, cn, dn: the number of the double functions is (4^2=) 16; and the quotients of these, or say fifteen of the functions, each divided by the sixteenth, are the hyper-elliptic functions of two arguments depending on the square root of a sextic function: generally the number of p-tuple theta-functions is $=4^p$; and the quotients of these, or say all but one of the functions, each divided by the remaining function, are the Abelian functions of p arguments depending on the irrational function y defined by the equation $F(x, y)=0$ of a curve of deficiency p. If instead of connecting the ratios of the functions with a plane curve, we consider the functions themselves as coordinates of a point in a (4^p-1) dimensional space, then we have the single functions as the four coordinates of a point on a quadriquadric curve (one-fold locus) in ordinary space: and the double functions as the sixteen coordinates of a point on a quadriquadric (two-fold locus) in 15-dimensional space, the deficiency of this two-fold locus being of course =2.

The investigations contained in the first part of the present memoir, although for simplicity of notation exhibited only in regard to the double functions, are in fact applicable to the general case of the p-tuple functions: but in the main the memoir relates only to the single and double functions; and the title has been given to it accordingly. The investigations just referred to extend to the single functions; and there is, it seems to me, an advantage in carrying on the two theories simultaneously up to and inclusive of the establishment of what I call the product-theorem: this is a natural point of separation for the theories of the single and the double functions respectively. The ulterior developments of the two theories are indeed closely analogous to each other; but on the one hand the course of the single theory would be only with difficulty perceptible in the greater complexity of the double theory; and on the other hand we need the single theory as a guide for the course of the double theory. I ac-

cordingly stop to point out in a general manner the course of the single theory; and in connexion with it, but more briefly, that of the double theory; and I then, in the second and third parts respectively, consider in detail the two theories separately; first, that of the single functions, and then that of the double functions. The paragraphs of the memoir are numbered consecutively.

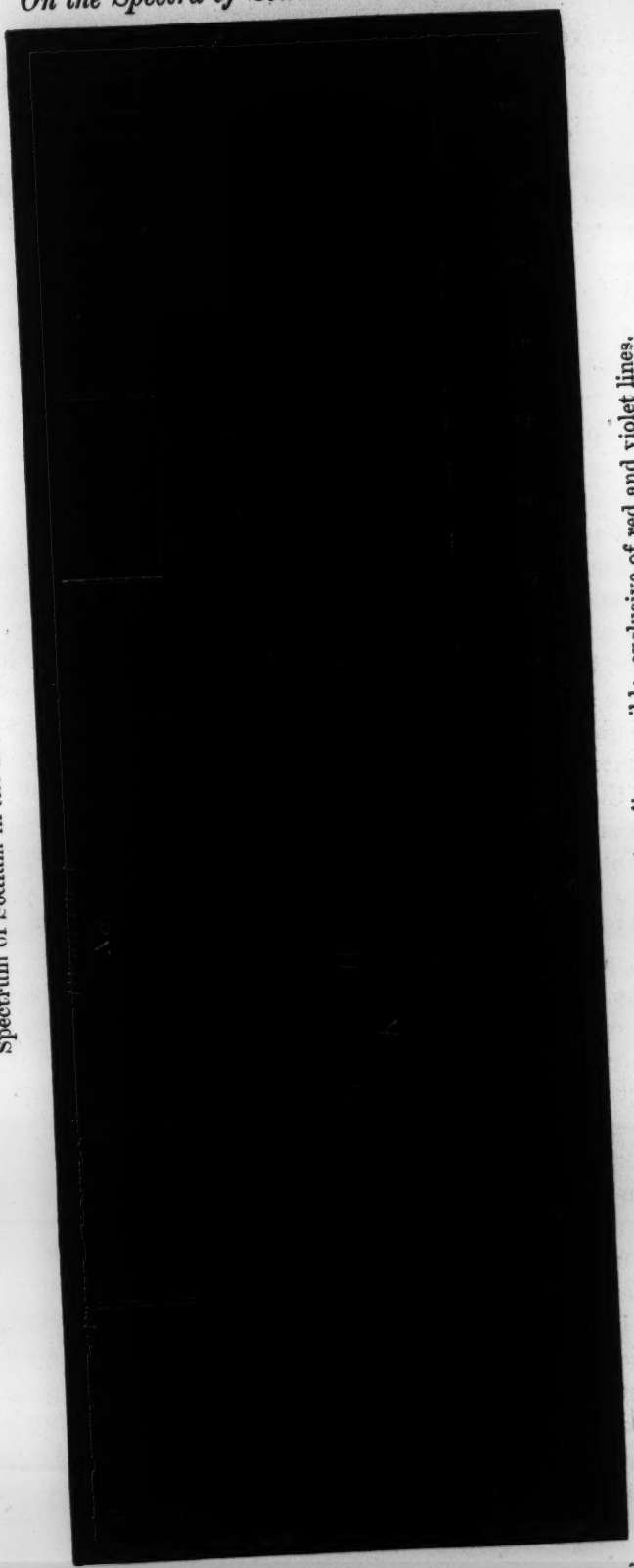
The definition adopted for the theta-functions differs somewhat from that which is ordinarily used.

V. "On the Spectra of Sodium and Potassium." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received November 20, 1879.

In the course of our observations on the reversal of the spectra of metals, we have many times had occasion to observe the spectra of sodium and potassium, and have noticed that they have been imperfectly described. M. Lecoq de Boisbaudran describes them more completely than anyone else; but as seen in the electric arc in one of the crucibles described by us in connexion with the reversals, there are in each spectrum several additional lines, which make the whole far more regular and symmetrical than they appear in his account. These additional lines are seen in the arc given by a battery of 25 Grove's cells as well as in that given by a dynamo-electric machine. They show best in a long arc, and as the vapours conduct well it is easy to draw out the arc of 25 cells to the length of an inch or more. We have generally used carbonates of the metals, sometimes chlorides.

In the case of sodium we have already noticed ("Proc. Roy. Soc.," vol. xxviii, p. 473) that the pair of lines next more refrangible than D (which we may for convenience call the third group) are accompanied by a fainter pair on their more refrangible side. Lecoq de Boisbaudran notices one only of these faint lines. By micrometer measurements of their distances from their brighter companions, we have deduced the wave-lengths 5673·6, 5668·6, for this pair. A line closely corresponding to the latter of these lines is to be found in Kirchhoff's map of the Solar Spectrum (1165·2).

The next pair of lines, or fourth group (5155, 5152), are sharply defined, and have no other line close to them; but the bright green pair, or fifth group (4983, 4982), are diffuse lines, usually seen as a band but noted by Lockyer to be a double line, and have a third line on their more refrangible side, of which we find the wave-length to be 4980·5. We feel sure that there ought to be a fourth line in this group, but have never been able to detect it. It is possible that two of the four lines may overlap, and so appear as one;



Spectrum of Sodium in the arc in a lime crucible.

Spectrum of Potassium in the arc in a lime crucible, exclusive of red and violet lines.

but the reversal of the brighter pair of the group seems to militate against such a supposition, as the third line has not been seen reversed. It is, however, much fainter than the other two.

The sixth group consists of a pair of lines sharply defined, for which we have found, by measurements of their distances from some bright manganese lines in their neighbourhood, the wave-lengths 4751·4, 4747·5. The first only of this pair is described by Lecoq de Boisbaudran. We have not seen either of them reversed, but there are lines in the solar spectrum which correspond closely to them and may therefore reasonably be attributed to sodium.

The seventh group is a pair of lines with diffuse edges, which we have seen reversed as fine dark lines in the middle of the diffuse blue bands. For these we deduced the wave-lengths 4667·2, 4663·7. The first only is described by Lecoq de Boisbaudran, and he assigns to it the wave-length 4670. By putting some titanic oxide into the crucible the titanium line 4666·5 was seen between the sodium lines, and we have no doubt that it is sodium which gives the winged appearance to the corresponding ray in the solar spectrum. The ray 4663·5 in the "normal solar spectrum" is probably the other ray of this pair, as the difference between the wave-length of this and that found by us is within the limits of error in our measurements of such diffuse lines.

The eighth group is a more sharply defined pair, for which by measurement of their distances from neighbouring barium lines we deduced the wave-lengths 4543·6, 4540·7. The lines 4543·9, 4540·2 in the "normal solar spectrum" probably represent this pair.

The ninth group is again a diffuse pair for which we deduced, by measurements from two bright manganese lines near them, the wave-lengths 4496·4, 4494·5. These are too diffuse for very exact measurement, but a bright line, which we could not certainly recognise as the manganese line 4495, appears nearly coincident with the more refrangible of them. Lines nearly corresponding to these are in the solar spectrum.

The tenth group is again a more sharply defined pair, for which, by measurements from calcium and iron-lines, we found the wave-lengths 4423, 4419·5. The lines 2656·7, 2664·9 of Kirchhoff's map of the solar spectrum may represent this pair.

The eleventh group is a very diffuse pair, sometimes seen as a continuous band dividing as the sodium evaporates, for which we found the wave-lengths about 4393, 4390. There is a line corresponding to the former in the "normal solar spectrum," and Kirchhoff's line 2709·6 may represent the latter.

The twelfth group is a diffuse but narrow band, which we have not seen divided. The middle of it has a wave-length about 4343, very nearly coincident with a line which we took to be an iron line, and rather more refrangible than a chromium line 4344·5.

The thirteenth group is a diffuse broad band nearly bisected by the iron line 4325.

The successive groups become fainter and more diffuse as they are more refrangible, at the same time the distance between successive groups diminishes. Their positions are shown on the accompanying diagram to a scale of wave-lengths. It is worthy of note that every alternate group is much more sharply defined than the others. Moreover, it is only the diffuse groups (3) (5) (7) which show reversal except the first group, in the orange, which, however, is more difficult of reversal than the others. The whole series, exclusive of D, looks very like repetitions of the same set of vibrations in a harmonic progression; the first (visible) term consisting of the six vibrations represented by the orange pair (6160, 6154) and the four lines of group (3); the next term of the five lines of the fourth and fifth groups, one of the six vibrations being now too faint to be seen; the next three terms, of each of which only four lines are visible, consisting of the sixth and seventh, and eighth and ninth, and the tenth and eleventh groups, and the last term of the two faint bands of the twelfth and thirteenth groups.

Simple harmonic relations can be found to subsist between some of the groups; for instance, the wave-lengths of the fifth, seventh, and eleventh groups are very nearly as $\frac{1}{15} : \frac{1}{16} : \frac{1}{17}$, but the whole series cannot be represented as simple harmonics of one set of six vibrations with any probability. The smallest numbers, which are nearly proportional to the reciprocals of the wave-lengths of groups (1), (4), (6), (8), (10), (12) are 81, 97, 105, 110, 113, 115; and these numbers are only approximately in the same ratios as the reciprocals of wave-lengths.

The potassium spectrum as seen in the arc, leaving out of account the two pairs of lines in the red and that in the violet, consists of a series of groups of four lines each, succeeding one another at shorter intervals and becoming fainter as they are more refrangible. They all are more or less diffuse, markedly more so on their less refrangible edges. They are shown on the accompanying diagram to a scale of wave-lengths. Lecoq de Boisbaudran gives a more complete list of potassium lines than other observers, but while the spark which he used brought out several lines which are not seen in the arc, the higher terms of the rhythmical series brought out very distinctly by the arc are not so well seen in the spark.

The first and least refrangible group of this series consists of the four lines to which Lecoq de Boisbaudran assigns the wave-lengths 5831, 5812, 5801, 5783. The second of these lines (5812) is much less strong than the others as seen in the spark. In the arc they are all nearly equal in brightness, but we have not seen the second line reversed.

Between the first and second group Lecoq de Boisbaudran gives a line which was not noticed by us in the arc.

In the second group the two middle lines are very close, and Lecoq de Boisbaudran gives only their mean wave-length. Measuring the distances of the middle from the extreme lines of the group, the wave-lengths of this quartet are 5355, 5338, 5334·5, 5319. These are all about of equal brightness, but we have not seen the third (5334·5) reversed.

The third group consists of the lines 5112, 5098, 5095, 5081. Of these the second is not given by Lecoq de Boisbaudran, and only the first has been seen by us reversed.

Three lines given by Lecoq de Boisbaudran between the third and fourth groups are not seen in the arc, or at least are not nearly so conspicuous as the groups here described.

The fourth group consists of lines for which we find the wave-lengths about 4964, 4956, 4950, 4942. Of these the first and last only are given by Lecoq de Boisbaudran as 4963 and 4936.

The fifth group has lines of which we find the wave-lengths about 4870, 4863, 4856, 4850.

The sixth group has lines of about the wave-lengths 4808, 4803, 4796, 4788.

The seventh group is too faint and diffuse to be distinctly resolved. The wave-length of the least refrangible edge is about 4759.

None of these last three groups are seen by Lecoq de Boisbaudran, and they are too diffuse for exact measurement; on the other hand, he gives several other lines which are not noticed by us in the arc.

As in the case of sodium the repetition of these quartets of lines at decreasing intervals with decreasing brightness and sharpness as they proceed from the less to the more refrangible, gives the impression of a series of harmonics; but the wave-lengths do not seem to be in a simple harmonic progression, though simple harmonic relations may be found between some of the groups.

VI. "On the Reversal of the Lines of Metallic Vapours." No. VII. By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received November 18, 1879.

The experiments of which the results are here given were all made in the Chemical Laboratory of the Royal Institution, with the powerful electric current from the Siemens dynamo-electric machine. Limestone crucibles similar to those before employed were in all cases used, but the opening through which the arc was observed was in general placed

horizontally, so that the axis of the collimator of the spectroscope was directed immediately on to the arc without the intervention of a mirror, and a small vertical opening was made for the introduction of the substances under observation, and was closed when not in use by a piece of lime.

With sodium carbonate the green pair wave-lengths 4983, 4982 were reversed, showing dark lines in the middle of the bright ones, the less refrangible of the two giving the stronger dark line. The sodium line given by Lecoq de Boisbaudran at wave-length 4670 showed as a diffuse blue band with a *pair* of fine dark lines in it, of which the stronger and more lasting was the less refrangible. The diffuse blue band resolved itself into *two* diffuse lines as the sodium carbonate evaporated, and the measurement of their positions in comparison with a conspicuous titanium line, which lies between them, and was made to show at the same time by introducing a fragment of titanic oxide into the crucible, gave for this sodium pair the wave-lengths 4667, 4664. The orange pair, wave-lengths 6160, 6154, were also seen reversed in like manner, but we failed to detect any difference in the strength or continuance of the dark lines in this case. These reversals of sodium lines were many times observed, and we noticed that the reversals of the other pairs continued after that of the red pair had ceased to be visible, that next after the disappearance of the dark lines in the red pair those in the diffuse blue pair ceased to be visible, then the dark lines in the green pair, and then those in the yellowish-green (5687, 5681). In some cases when a large quantity of sodium carbonate was put into the crucible a curious double reversal occurred. In the middle of an enormous dark expansion of D a bright yellow band appeared, which in turn had a narrower dark band, or a pair of dark lines, in its middle. A similar double reversal of the lithium blue line occurred so far as to show a bright line in the middle of the dark one. Of the two violet lines of potassium we observed that the more refrangible remained reversed longer than the other.

In addition to the reversals of calcium lines before observed by us, we have noticed the reversal of all the more conspicuous calcium lines of the G group, viz., those wave-lengths—4282, 4289, 4298, 4307, 4318, besides 4302 before noted, also that of 4379, and all three lines of a triplet 4585, 4581, 4578. The finer lines, wave-lengths 4434·3, 4454·5, slightly less refrangible than the strong lines 4434, 4454, were reversed, but only when one of the poles was a bar of iron, instead of carbon. The strong lines just mentioned were expanded so as to cover their neighbours, and all four lines were seen black against the bright background in the positions and of the same relative strengths as when bright.

When strontium chloride was put into the crucible the lines with the following wave-lengths, besides those before noted, were observed

reversed—4305, 4865, 4892, 5223, 5225, 5228, 5480, 5503, 5522, and momentarily only, 5155, 5238, 5256. Besides these, many dark bands were observed in the less refrangible part of the spectrum, of which three, with wave-lengths 5920, 6035, 6060, appear to be identical with bright bands ascribed to strontia, and one, with wave-length 6597, is identical with a bright line given by strontium chloride.

Manganese, introduced as sulphate, gave with facility the violet triplet, 4029, 4032, 4033 as dark lines on the continuous background. The bright blue lines of manganese were not, however, reversed until some metallic magnesium was introduced. This brought out the reversal of the lines, wave-lengths 4753, 4783, and 4823, the last being the most easily reversed of the three.

Lead introduced in the metallic state gave a reversal of the violet line, wave-length 4058, which Cornu had previously seen reversed, but this reversal was far better seen, becoming a wide black band, when the lead was introduced as an alloy with zinc. Probably the lead vapour was not so rapidly oxidised when mixed with zinc, and a thicker, if less dense, stratum interposed between the arc and the spectroscope. When lead ferrocyanide was used, not only the line above mentioned was reversed, but also much less strongly, a line near it, wave-length 4062.

With zinc, only the less refrangible two of the three blue lines, wave-lengths 4810, 4721, were seen reversed. The very bright lines, wave-lengths 4924, 4911, seen in the spark between zinc poles, were not seen by us at all in the arc, resembling in this respect the magnesium line, wave-length 4481, and the cadmium lines, wave-lengths 5377, 5336.

When cadmium was put into the crucible the lines, wave-length 5085, 4799, and 4677, were reversed, not the line, wave-length 4415. With a large dose of cadmium the red line, wave-length 6438, was once seen reversed for an instant only.

With silver, besides the reversals before observed by us, the line, wave-length 4053, showed a dark line in the middle of its expansion as noticed by Mr. Lockyer, but we could see no reversal of the line, wave-length 4208. Instead of the reversal of this line we observed that a second bright line came out close to it, rather diffuse, and about midway between the line 4208 and the calcium line 4215. By measuring the intervals and interpolating, we deduced 4211·3 for the wave-length of this silver line, which we could see, though it was fainter, in the less powerful current of a De Meritens' machine. This second line coming out near the other silver line gave the appearance of a reversal in the middle of a diffuse line, but besides the measurements made with a micrometer we assured ourselves of the fact of there being two lines by watching the fading of the second line as the silver evaporated. The use of an alloy of zinc with silver did not alter

the appearance of these two lines, or bring out a reversal of either of them. We failed to see any line of silver either bright or reversed with wave-length about 4240, as mentioned by Cornu, whether the silver were used pure or alloyed with zinc or with lead. With the carbons arranged vertically as shown in our last communication on this subject, p. 472 (vol. xxviii), and the light viewed through the perforated carbon, silver gave a channelled spectrum as described by Lockyer and Roberts. As this channelled spectrum was not seen with silver in any other arrangement of our crucibles, we are led to attribute it to a comparatively cool condition of the silver vapour ascending the carbon tube, a condition of near approach to a state of liquefaction.

Having observed that lines frequently came out with mixtures which were not visible when the separate ingredients were used, we tried a few amalgams. None of these showed any reversals of the mercury lines. But an amalgam of bismuth gave readily the reversal of the bismuth line, wave-length 4722, and with more difficulty that of the line, wave-length 4119.

Antimony did not appear to give any lines, or none easily distinguishable, in the arc.

With copper we observed the reversal of two lines only, wave-length 5105, 5153.

Iron introduced as metal, or as chloride, in the usual way, gave us no reversal; with an iron rod used as positive pole instead of one of the carbons, we succeeded in getting the reversal of one line, wave-length 4045, which expanded and showed a fine dark line in its middle; but by passing an iron wire into the arc through the positive carbon, which was perforated in the manner described in our last communication on this subject (p. 472), for the introduction of gases into the arc, and pushing in the wire slowly as the end burned away, we succeeded in reversing several of the brightest of the iron lines. The three violet lines, wave-lengths 4045, 4063, 4071, were the first to be reversed. They all expanded before showing reversal, and the order of reversal was that of refrangibility. Besides these the lines, wave-lengths 4957, 4920 (the stronger and less refrangible of a pair), 4404, 4383, 4325, 5040, 4307 (G) were reversed. These last two lines could not be certainly distinguished from calcium lines, as the last is almost if not quite coincident with a calcium line, and the other is too close to a calcium line to be distinguished without more exact measurement than the time during which it could be observed would allow, but we judged from their relative intensity that they were the iron lines. The line, wave-length 4415, was not seen reversed when the other bright lines near it were reversed.

Nickel, whether put into the crucible in the old way, or fed into the arc in small fragments filling a platinum tube which was passed through a perforated carbon pole, gave no definite reversal of any of

its lines; nor did cobalt, even when a bar of cobalt was used as the positive pole.

Tin, palladium, and platinum gave no reversals.

It is worthy of remark in regard to the difficulty of obtaining substances chemically pure, that we found that carbon poles which had been for some hours ignited in a current of chlorine and further intensely heated in the arc, while chlorine was passed through perforations down their axes, still showed in the arc, of course without any crucible being employed, a multitude of lines, amongst which the so-called carbon lines and those of calcium and iron were conspicuous.

December 1, 1879.

ANNIVERSARY MEETING.

THE PRESIDENT in the Chair.

Mr. Perkin, for the Auditors of the Treasurer's Accounts, on the part of the Society, reported that the total receipts during the past year, including a balance of £916 14*s.* 7*d.*, carried from the preceding year, amount to £9,048 5*s.* 1*d.*, and that the total expenditure in the same period, including purchase of stock, amounts to £7,783 18*s.* 5*d.*, leaving a balance at the Banker's of £1,222 12*s.*, and £41 14*s.* 8*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Peyton Blakiston, M.D.
Edward Blore, LL.D., F.S.A.
Charles Brooke, M.A., F.R.C.S.
John Allan Broun.
George William Callender, F.R.C.S.
James Robert Christie.
W. Kingdon Clifford, M.A.
George William Drory.
Isaac Fletcher, F.R.A.S.
William Froude, M.A., M.I.C.E.
A. H. Garrod, M.A.
John Benjamin, Baron Heath, F.S.A.
Sir Rowland Hill, K.C.B., D.C.L., F.R.A.S.
Rev. Philip Kelland, M.A., V.P.R.S.E., F.C.P.S.

Major-General Sir Thomas Aiskew Larcom, Bart., K.C.B., R.E., M.R.I.A.
Sir John George Shaw Lefevre, K.C.B., M.A., D.C.L.
Sir Thomas Maclear, F.R.A.S.
Anthony Mervyn Story Maskelyne, M.A.
Sir James Matheson, Bart.
J. Clerk Maxwell, M.A.
John Miers, F.L.S.
Charles Murchison, M.D., LL.D.
William Wilson Saunders, F.L.S.
Thomas Sopwith, M.A., F.G.S.
Arthur Hay, Marquis of Tweeddale.
Sir Richard Rawlinson Vyvyan, Bart.
John Waterhouse.
Samuel Charles Whitbread, F.R.A.S.
Bennet Woodcroft.

On the Foreign List.

Heinrich Wilhelm Dove.
Johann von Lamont.

Fellows elected since the last Anniversary.

Prof. John Anderson, M.D.
Rev. Miles Joseph Berkeley, F.L.S.
Sir Henry Bessemer.
Prof. Alexander Crum Brown, M.D.
Walter Lawry Buller, Sc.D., F.L.S.
Right Hon. Richard Assheton Cross.
George Howard Darwin, M.A.
Prof. Joseph D. Everett, M.A., D.C.L.
Prof. Francis Stephen Bennet François de Chaumont.
Prof. George Downing Liveing, M.A.
George Matthey, F.C.S.
George John Romanes, M.A., F.L.S.
Arthur Schuster, Ph.D., F.R.A.S.
Prof. Harry Govier Seeley, F.L.S.
Benjamin Williamson, M.A.
Thomas Wright, M.D., F.G.S.

On the Foreign List.

Arthur Auwers.
Luigi Cremona.
Jean Louis Armand de Quatrefages.
Georg Hermann Quincke.
Theodor Schwann.
Jean Servais Stas.

The President then addressed the Society as follows :—

Gentlemen,

In the spring it is our duty to elect into the Royal Society our annual complement of new Fellows, and at our early summer meetings we admit to our ranks the young and vigorous in the career of science. But in the autumn, or fall, as on the other side of the Atlantic it is so aptly termed, it is our custom to recount the names of

those who have dropped from our list. Some of these have fallen in a plenitude of years, and in maturity of mind: others in that early stage of youth where insight and imagination, as yet scarcely differentiated, have both lent their aid to the first flights of thought. Which of these classes we can best afford to spare is hard to say; but whether it be the one, or whether it be the other, it is certainly the case that the number of those whom we have thus lost since our last anniversary has been unusually great; it has in fact reached a total of twenty-nine on the home, and two on the foreign, list. Many of them find a place among the obituary notices which appear from time to time in our Proceedings; but there are a few, of whom, from personal knowledge, or from special interest in their works, I am led to make additional, although passing, mention here.

In looking over the list we are reminded in a striking manner of a fundamental difference between the Royal Society and the Academies of the Continent, a difference which may perhaps be the best described by the term comprehensiveness. For, beside the class of Fellows selected, in accordance with our recent legislation, from the members of the Privy Council, it has always been our custom to gather into our ranks not only men of eminence in science proper, and in subjects which border on it, but also men of distinction in other paths of life, provided that they have followed those paths on principles which are analogous to our own, and which by no undue strain of the analogy may themselves be called scientific.

In illustration of this remark, I might point in the present list to the man of letters, to the architect, to the politician, to those who have honourably served in various departments of the public service, to the man of wealth who has turned his large means to large-minded purposes for the welfare of the people. And although the act of erasing them from our list marks our loss, yet the fact of having once reckoned them among our number is in itself a gain, and must help to enlist the sympathies of the world outside in our special function, viz., the promotion of natural knowledge, while at the same time it tends to enlarge our own.

To mention briefly a few of these: in Sir James Matheson we have lost a wealthy and enlightened member, who devoted much of his time, his energy, and his means in promoting the welfare, both moral and intellectual, of the people among whom he made his home.

In the Marquis of Tweeddale we have an instance, happily not singular, of one who, without any professional connexion with the subject, contrived amidst the distractions of active service to lay the foundations of a solid knowledge of one branch of science; while in later years he became an active collector and the author of valuable cnotributions to the publications of the Geological Societies over which he presided.

In the category of men of cultivation and leisure, who have turned their attention with good purpose and success to scientific pursuits, we may fairly reckon our late Fellow John Waterhouse, of Halifax.

Sir Thomas Larcom was one of a long series of distinguished men who have been elected Fellows of the Royal Society from the Corps of the Royal Engineers. Travelling beyond the strict limits of his professional career, he endeavoured to give to his official work a wider range. And it was due to suggestions emanating from him that the Irish Survey was so extended as to make it the opportunity for collecting a great variety of local information, history, language, and antiquities, and that the map became the admiration of scientific travellers.

Mr. Bennet Woodcroft's name will always be associated with the foundation of that important department the Patent Office, with its Library, and Museum, of which he was the first executive officer.

Professor Kelland and Mr. Brooke were among our veterans in the Society, and many of us will long recollect the lively interest in science which the latter showed during his frequent attendance at our evening meetings.

In Mr. W. Froude, to whom one of the Royal Medals was awarded in 1878, the Society, the public service of the country, and science in general have sustained a loss which at one time would have been irreparable, and which even now, when his work has become an established science, is difficult to replace.

Of Professor Clifford, and the gap which his death has caused among our friends and the world of science at large, I know not how to speak. His mathematical papers are being collected by a careful and trusty hand, while his philosophical remains have been given to us by one who knew and loved him as he deserved. To the same friend we owe the memoir of his life, written indeed so far as that life can ever be written, but reminding us also at every line that his life was one the full story of which will always seem but half told.

If in Professor Clifford we have lost one of our youngest, in Sir J. G. Shaw-Lefevre we have lost one of the oldest of our mathematical Fellows. Born before the present century, and Senior Wrangler at Cambridge before many of us were in existence, he seems to belong to a past age. But through a long and honourable course of unusually varied and responsible public life he always retained his interest in mathematics, and, himself no mean geometer of the old school, he had in his later years projected and actually begun a new edition of the works of Archimedes. Of his labours in connexion with University College, and afterwards with the University of London, all who are interested in liberal education will retain an appreciative and grateful memory.

Our last loss, that of Professor Clerk Maxwell, has been in some respects the greatest. In him the full maturity of a mind which had

suffered no check or shadow of abatement, was devoted to the foundation of a more thorough knowledge of Molecular Physics and of Electrical Laws than has hitherto been attained. The clear insight, the keen criticism, the just and liberal appreciation of the views of others, evinced both elsewhere and especially in his Reports to the Committee of Papers, will long be remembered; while the more general side of his character, exemplified in the numerous scintillations struck off with his pen, will be no less cherished by those to whom his scientific writings were as a sealed book.

To replace such a man seems at first impossible; but we should be doing scant justice to his memory if we did not believe that the good seed which he so liberally sowed would bear such fruit that in the time of need there should be no lack.

The only change which I have to record among our staff is that in the post of Clerk and assistant in the library. Mr. Wheatley, who had held that office for eighteen years, has been appointed Assistant Secretary to the Society of Arts. His place has been supplied by Mr. Rix, who will doubtless justify the high recommendations by which his candidature was supported.

My predecessor in office wisely made the anniversary address the occasion of communicating to the Fellows at large a summary of the business which, apart from that which is brought before our evening meetings, has occupied the attention of the Council during the current year. This practice I purpose to continue.

The subjects which have come before the Council and its Committees during the past year, although perhaps less multifarious, have not been less important than those with which they have had to deal in former years. They may perhaps be conveniently classed under the following heads:—(1) Property; (2) Regulations and Procedure; (3) the Government Grant and Fund; (4) Matters referred to us by the Government; (5) the Publications of the Society.

(1.) Property. The Fees Reduction Fund.—This has received a few, but not large, contributions. It was calculated, as stated on a former occasion, that an investment of the surplus, after providing for the loss of income arising from the abolition of entrance fees, and the reduction of annual payments from £4 to £3, would render the fund permanently self-supporting. It will, however, be in the recollection of the Fellows that two objects presented themselves to the mind of the Council when the fund was originated, viz., the reductions above mentioned, and a provision for the expenses of our publications; and that neither of them has been wholly abandoned. Although the Fund, as now constituted, is primarily directed to the first of these objects, yet it has been hoped that, through accretions which may arise from time to time, the second purpose may eventually be also furthered. And bearing in mind the increasing demands upon the Society's income

from the increase of scientific communications, and the larger expense now incurred for illustration, it is much to be desired that augmentations to the Fund may be actually made.

As the period seems now to have arrived for improving the value of the estate at Acton belonging to the Society, by applying the whole or a part of it to building purposes, it may be well to say a few words with regard to this property.

The capacity to purchase and hold lands, the Statute of Mortmain notwithstanding, was granted to the Society by charter soon after its foundation, and subsequently, under George the First, a special licence to purchase in mortmain to the yearly value of £1,000, and to receive lands, tenements, rents, or hereditaments by will or any deed of conveyance, was granted by Letters Patent. In 1731, when the Society had some money to invest, an estate of a good title near Fulham was mentioned as a suitable investment by the President, Sir Hans Sloane; and in June, 1732, a freehold farm, the estate of Mr. Pannet, of Kensington, having a dwelling-house and out-houses, with about forty-eight acres of arable land, was recommended for purchase by Mr. West. In the following month the purchase of this estate at Acton was settled, the purchase-money being £1,600, but Mr. Pannet agreeing to lay out £30 on repairs. The total rent at that time was £65 per annum and a quit-rent of £1 4*s*. In 1772 and 1774 the house and four acres of land were sold for £620. Another plot was sold in 1792. In 1858 the estate, which at first consisted of many separate plots, was consolidated under an award of the Enclosure Commissioners, and within the last few years other portions have been sold to the North and South-Western Junction Railway, and for the purpose of erecting a church, parsonage, and schools. The sums received from these various sales have been considerably in excess of the £1,600 originally paid for the estate, so that the thirty-four and a half acres which, until within the last few months, remained the property of the Society, and produced an annual rent of £170 per annum, had practically cost the Society nothing. During the present summer the railway company, being desirous of constructing a station upon the estate, purchased somewhat more than an acre of our land; but it has been thought desirable to devote a portion of the purchase-money to buying a strip of land to give access to a part of the estate, the only approach to which was by a level crossing over the railway, which the company wished to close. The lease of the whole property will shortly fall in, and, in re-letting it, provision will have to be made to enable the Society to take any portion of the land which may be required for building purposes. A considerable part of the estate is well intersected by roads, and offers desirable frontages, but in order to make the whole of these available some outlay will be necessary, and it may perhaps be

found desirable to lay out additional roads at the expense of the Society. There appears, however, to be every probability that, by selling and letting various plots on the estate, the eventual income of the Society from this source may be materially increased.

Besides the estate at Acton, the Society is possessed of a small grazing farm at Mablethorpe, in Lincolnshire, and although this has been let to a substantial tenant at a moderate rent, it seems not improbable that our income so arising may, like that of many others, be affected by the prevailing agricultural depression. The amount, however, is in this case comparatively small.

Legacies.—Under this head I have the pleasure to mention two important contributions to the Society's funds. First, an unconditional bequest of 1,000*l.* by the late Mr. Sydney Ellis, of Leicester; and secondly, a legacy by the late Sir Walter Trevelyan, "the interest of which is to be applied to the promotion of scientific research." The object of this last bequest appears to be almost identical with that of the Donation Fund; and the name of our benefactor may not improperly be associated with those of Wollaston the founder, and of others who have from time to time contributed to augment that fund.

The Royal Society, as is well known, possesses a rather extensive gallery of portraits, almost exclusively of Fellows of the Society, but among them also a fine painting of Lord Chancellor Bacon. Many of these portraits, however, have, through the lapse of time, begun to show signs of decay. Acting under the advice of Mr. F. W. Burton, F.S.A., Director of the National Gallery, the Council has entrusted the pictures which seemed most to require attention to the care of Mr. Dyer, of Orchard Street, who is now engaged upon them. Some of the portraits require lining, and others cleaning, or partial restoration. As will be seen from those which have been returned to their places, the work appears to have been done in a satisfactory manner. The present appearance of the pictures has been much improved, and it is hoped that these interesting portraits of those who have gone before us may now be passed on in an unimpaired condition to future generations.

Among other acquisitions to the Society's property, I would call the attention of the Society to the collection of no fewer than 973 portraits of Fellows of the Royal Society, formed by the late J. P. Gassiot, Esq., F.R.S., and bought during the past year. The collection consists mainly of engravings; many of which are of great artistic merit, and in excellent condition. It is to be hoped that the existing Fellows will aid in completing and continuing this interesting series, by contributing engravings or even photographs of themselves or of other Fellows, as occasion may offer.

Mention should also be made of a bequest by the late Mrs. Faraday of "a book containing the numerous diplomas and other documents of

honour and compliment conferred on, or addressed to her late husband, Michael Faraday."

(2.) Regulations and Procedure.—At the last anniversary your late President explained the changes then in progress for the abolition of entrance fees, and for reduction in the annual payments made to the Society by the Fellows. I have now only to add that the alterations in the Statutes, necessary for carrying this out, have been completed.

During the past year a small but perhaps not unimportant change in the mode of dealing with the papers to be read at the weekly meetings has been made. This consists first, in deciding a week earlier than heretofore, what papers should be advertised for reading; and secondly, in reading each week as many as practicable of those in hand, so as to leave as few as possible to stand over. The weekly journals are now able to announce to the public the papers which will be read at the Royal Society (as has in fact long been the case with other Societies) during the next week. But the main object of this arrangement has been early publication; that is to say, publication both in its technical sense of reading before the Society, and in its more widely accepted sense of appearance in the Society's Proceedings. When this was first proposed, it was feared there would soon arrive a period of scientific famine, and that occasions might occur when the Society would meet with no papers before it. Whether this would be so great a calamity as was at first imagined is still an open question, for such has been the scientific fertility of the season, that the threatened catastrophe has never yet actually occurred. But, more than this, your President was supported in any anxiety which he may have felt on this score, by an assurance on the part of the Secretaries that in the event of no other communication reaching us from other quarters, one or other of them would contribute something from their own store. And although our stock of papers has never reached a famine level, our two principal Secretaries have fulfilled their promise by anticipating it. Having regard to this prompt action on their part, I doubt not that the Society will agree with me in thinking that the prospect of a dearth of papers has proved to be productive of advantages rather than the reverse.

But so far from suffering by a deficiency of matter, we have more often found our difficulties in the number of papers to be read in a single evening. And on such occasions the Secretaries have been good enough to take especial pains to make themselves masters of the contents of the papers, and to communicate in a few words to the meeting the substance of each. It is, I believe, not too much to say that the "reading" of papers carried out in this way has been the most agreeable and instructive, and has been particularly provocative of intelligent and pertinent discussion. The previous preparation necessary to effect this must, I know, entail considerable

trouble on the part, of our already hard-worked Secretaries; but I am equally sure that all those who have been present at those their *vivâ voce* expositions will join with me in an expression of our sense of the obligations which they have thus conferred upon us.

Of the utility of printing copies of the abstracts of papers in time for the meetings, and of circulating copies to the authors themselves, I myself entertain no doubts. But it must still be admitted that full use of these facilities has hardly yet been made, and that the discussions on the papers have not apparently been much promoted by them. I trust, however, that the authors of papers and others who may attend our weekly meetings will endeavour to make every use of facilities which are afforded at a not inappreciable cost to the Society.

There is a possible alteration in our arrangements which has often appeared to me to be worthy of consideration, and which from conversations with some of our Fellows appears to meet with sufficient support to justify my bringing it before this our anniversary. I refer to the hour at which our weekly meetings are held. Hitherto, in accordance with the usage of Scientific Societies in London, we have met in the evening. But changes in the habits of society, and the increasing distances from Burlington House at which many of our Fellows reside, seem to render a large weekly attendance difficult. On this account it appears to me desirable to inquire whether an afternoon hour might not better suit the convenience of our members. In that case, I should suggest 5.0 P.M.; and as our meetings seldom extend to two hours in duration, it would generally be practicable for Fellows to reach home by about seven o'clock.

I should, however, much regret if any such change should deprive us of the pleasure and of the substantial benefits arising from the conversations in our tea room. And, having this in view, I would suggest that on Thursday afternoons tea should be prepared an hour before the meeting, so as to give opportunity for conversation to such of our Fellows and their friends as might be disposed to avail themselves of it.

Under such an arrangement for our weekly meetings, it would doubtless be necessary to make further provision for the Meetings of the Council and of the Committee of Papers. On this point I should desire the advice of the Council and the suggestions of individual Fellows. But in the meantime I would venture to point out that it might be sufficient to transfer either the Council or the Committee of Papers, or both, from the Thursday to the Wednesday. And although the former plan would involve bringing the Members of Council rather more often together, the total time occupied would not be increased; and it might be not without advantage that the Committee of Papers should be held on an independent day.

These changes, if adopted, would require the alteration of the Statute relating to the hour of meeting. But if the suggestion were

adopted before the end of the year, there would still remain nearly half the Session of our Society after complying with the necessary formalities.

At the risk of becoming tedious on matters of internal detail, I venture to mention another subject. It has often been suggested that our weekly meetings might be rendered more interesting if the communications were more often accompanied by experiments, or by other modes of optical illustration. The Council has hitherto met these requirements by supplying, from time to time, such appliances as appeared necessary. But that important element, the electric light, and batteries on a large scale, have generally been avoided, on account of the inconveniences attending them. It has, however, been thought that authors would be much encouraged to illustrate their communications experimentally if the main appliances were known to be always ready to hand.

Again, the mode of lighting our meeting-room by means of sunlights has proved inconvenient to many of our Fellows, on account of its heat and glare; and it is considered undesirable to adopt ordinary gas-burners in its stead for fear of injury to the pictures. We are, however, no longer driven to this alternative, as we may now look to the electric light as a possible mode of illumination.

These considerations have led me to make an offer, as I now do, to the Society, of a gas-engine of eight horse-power, which, in the opinion of those best qualified to judge, will be amply sufficient both for experimental illustration and for illumination. And I have much pleasure in adding that, on hearing of this offer, our Fellow, Mr. Siemens, immediately expressed his wish to add a dynamo-machine, or rather a pair of such machines, of improved construction (one for alternate, the other for direct currents), the principle of which he had already contemplated bringing before the Society. The other requisites, such as an optical lamp and a few instruments of frequent use, will doubtless soon follow. But, in proposing thus to promote experimental illustration of papers read before the Society, I think it right to add that I do not contemplate, nor do I think it desirable, that the Society should in any sense establish a laboratory; all that is here intended is, that the main appliances for illustration should be found ready to hand here, while the special apparatus would be furnished by the authors themselves.

If these proposals meet with the approbation of the Society, the experiment may shortly be tried; and I have little doubt that, with the advice of the Council on matters of detail, the undertaking may be brought to a successful issue.

(3.) *The Government Grant and Fund.*—The particulars of the grants made from these two funds will be published as usual in our "Proceedings," with the report of the Anniversary Meeting. Some of the

results are already before you in the "Philosophical Transactions" and "Proceedings," and others may be expected to follow. These and similar publications by other scientific societies must serve as the main basis for forming a judgment of the value of the grants in question, and especially of that which is technically known as the "Government Fund." But whatever be the final verdict on this head, I feel sure that there will be but one opinion as to the impartiality and judgment with which the Committees, to which the matters were referred, have discharged their onerous duties. We are now entering the fourth of the five years to which, as will be remembered, the Treasury limited the term of the experiment. It is, therefore, desirable that the minds, not only of the Council, but also of the Fellows generally, should during the present year be turned to the question, whether it is advisable, in the interests of Science, that the Fund should be maintained: and if so, whether in its present or any altered form?

(4.) Among the matters referred to by the Government two especially require mention. In November last the Secretary of State for the Home Department addressed a letter to the President of the Royal Society, in which, after alluding to the great sacrifice of life in colliery accidents, he said, "I cannot help thinking that science may once more come to our aid in this very practical question; and I gladly consult you, in the first instance, as to the best means of obtaining further scientific information in the possession of scientific men, likely to be of practical utility in the prevention of accidents in mines." This letter was referred to a Committee, and on their recommendation the Council directed a reply to be made to the Home Secretary, suggesting an inquiry into the subjects (indicated somewhat in detail in the letter) bearing upon the causes of explosions in coal mines, and upon their possible prevention. Acting on this letter, Mr. Cross moved the appointment of a Royal Commission on the subject, and requested the President and Council to nominate four members, one of whom should act as Chairman of the Commission. The Fellows nominated in accordance with this request were, Mr. Warington Smyth as Chairman, Professor Abel, Professor Clifton, and Professor Tyndall. The Commission is now actively engaged upon the inquiry.

In May last the Secretary of State for India asked the advice of the Royal Society on the question of deputing to this country Major J. Herschel on the subject of pendulum observations. The subject is one in which the Royal Society has on more than one occasion taken an active interest; and a reply, prepared by Professor Stokes, of which the following is an extract, was sent:—"There are two independent methods by which a knowledge of the exact figure of the earth may be arrived at; one by measurement of arcs of the meridian,

or arcs of parallels of latitude, in different places; the other by measuring by means of the pendulum the variation of gravity from place to place, from whence the figure of the earth may be deduced by an extension of Clairaut's theorem.

"A very elaborate investigation of the problem as deduced from geodetic measurements has been made within the last few years by our Fellow Captain (now Colonel) Clarke, R.E., in connexion with the great Trigonometrical Survey of the United Kingdom, but including results of the geodetic operations, so far as they bear on this problem, undertaken by other nations and in other parts of the world.

"The results as to the figure of the earth, deducible from pendulum observations that had been then executed, were discussed by the present Astronomer Royal in an article published in the 'Encyclopædia Metropolitana.' But thirty years have passed since the publication of that work, during which time very important contributions to our knowledge of the variation of gravity over the earth have been made by different nations. Among them the elaborate series that was carried on in connexion with the great Trigonometrical Survey of India occupies a prominent place. It appears, therefore, very desirable that the conclusions deducible from our present stock of pendulum observations should be exhaustively discussed."

Major Herschel is on his way to England, to carry out the proposed work.

(5.) The Publications of the Society.—The Catalogue of Scientific Papers.—The second volume of the supplementary decade, viz., 1863–73, has been brought to a close, and copies are now in the hands of the Fellows and the public. It exceeds in bulk any of the earlier volumes of the work, and extends to 1,310 pages. In this supplement, 343 additional scientific serials have been catalogued, making the total of such serials now comprised in the whole no less than 1,938. The donation list for this volume has been the same as that for former volumes, with the addition of a few Societies and Institutions sanctioned by the Treasury at the recommendation of the Council. The Fellows have the right to purchase the supplement at the same reduced price per volume as the original work. The Council has authorised the preparation of titles for another decade; and some progress has already been made in the work.

Apart from the Catalogue of Papers, the main strength of the *Library* of the Royal Society consists in its collection of the publications of scientific Academies, and Societies, and of scientific journals, unrivalled anywhere in this country, except perhaps in the British Museum. It has always been held that the first duty of the Library Committee is to maintain, and wherever possible to increase, our efficiency in this respect. As regards our general collection of books, the Committee and Council are of opinion that it will be best to

restrict their purchases, as far as possible, to recognised standard works.

The change of printers, mentioned last year in the address of my predecessor, has as then hoped effected considerable economy; and I should not allow the present occasion to pass without expressing the sense of the officers of the zeal and attention shown by Messrs. Harrison to the business of the Society. The change in question occasioned the removal of our stock of "Philosophical Transactions" and "Proceedings" from the warehouses of our late printer, and as it would have been alike unnecessary and expensive to hire other warehouses at our present printers or elsewhere, we have taken the stock into our own custody. This has been the more easily effected, owing to the transference of the stock of the Catalogue of Scientific Papers to the Stationery Office. The change has been made the occasion of thoroughly re-arranging our stock of "Transactions" and "Proceedings;" a process which will always be necessary at intervals of some years, in order to keep the stock in a satisfactory condition.

An extra volume of the "Philosophical Transactions," (vol. 168) has been issued, in which the observations made by the naturalists who accompanied the Transit of Venus Expeditions to Kerguelen's Land and Rodriguez, and descriptions of their collections by persons specially acquainted with the several subjects, are brought together. The volume is divided into four sections, viz., the Botany and Zoology of each of the two islands respectively.

The Botanical collections from Kerguelen's Land, worked out by Sir J. D. Hooker, Mr. Mitten, the Rev. J. M. Crombie, Dr. Dickie, P. F. Reinsch, and the Rev. M. J. Berkeley, have added largely to our knowledge of the Cryptogams, especially the Algæ. In particular the American affinity of the Kerguelen Flora, previously established by Sir. J. D. Hooker, in his examination of the flowering plants, is proved to be also very strongly manifested in the Cryptogams.

Of the Zoological collections from Kerguelen's Land, those of the Mollusca, Crustacea, Arachnida, and Insecta, have yielded the greatest number of novelties; the two former offering distinct evidence of the affinity of this Fauna (as of its Flora) with that of South America. The Arachnids and Insects of the southern extremity of America are unfortunately too little known at present to admit of a comparison with the highly interesting new forms discovered by Mr. Eaton, and described in this section.

In estimating the affinities of the Flora and Fauna of Rodriguez, the authors were under great difficulties owing to our imperfect knowledge of the plants and animals of the other Mascarene Islands. But almost all their observations point strongly to the conclusion that the present animals and plants are the remains of a once more extensive Flora and Fauna which has been gradually broken up by geological

and climatic changes, and which more recently has been greatly interfered with by the agency of man. The greater portion of the botanical section has been undertaken by Dr. Js. Bayley Balfour, who himself collected the materials. He estimates the total number of plants found at present in Rodriguez at 470 species, viz.:—297 Phænogams and 173 Cryptogams; and he establishes the remarkable fact that of the Phænogams not less than 108 are to be regarded as introduced into the island, and that 189 only are indigenous. Great interest attaches to remains of the extinct cave-fauna of Rodriguez; and the collection made by Mr. Slater, supplied materials for three papers by Mr. E. Newton, Mr. J. W. Clark, and Dr. Günther. With regard to Zoology, the marine collections were those to which least interest was attached, as they consisted principally of common forms spread over the whole of the tropical Indian Ocean. On the other hand, our knowledge of the Terrestrial Invertebrates received considerable additions, especially in the Myriopods and Arachnids, Coleoptera and Turbellaria, described by Mr. A. G. Butler, Mr. C. O. Waterhouse, and Mr. G. Gulliver.

The Papers in General.—The papers presented to the Society, and read at our evening meetings, have been more numerous than in any previous year of our existence, and have during the last twelve months reached a total of 118. Some of them appear to have excited unusual interest among the Fellows and their friends; for, on more than one occasion our meeting-room was filled to an almost unprecedented degree.

But, beside the interest attaching to their reading and discussion, the papers themselves have offered some very striking features. It would be as invidious to attempt, as it would be impossible to establish, any general comparison of merit among so varied a collection of memoirs; but I may still be permitted to take this opportunity of expressing my own impressions of a few which fall, more or less, within my own range of study.

In purely experimental research, that is, in experiments guided by a clear conception of what was wanted to be done, and executed with adequate instrumental appliances and with the highest manipulative skill, we cannot but be struck by the assiduity and success with which Mr. Crookes has continued his labours. He has now brought to a termination his remarkable series of papers on Repulsion resulting from Radiation; and has already struck out into a new region, viz., the study of certain Electrical Phenomena which appertain especially to high vacua. In the vacua to which he has now turned his attention, the exhaustion has been carried even beyond that in which the phenomona of the Radiometer are produced; and it is by a legitimate and sagacious step that he has now occupied a complete field of inquiry intermediate to the most extreme vacua ever

attained, and those in which the stratified discharge is displayed. Of the delicacy and beauty of the experiments it is not necessary here to speak, for they have been already exhibited here, as well as to more than one audience on a large scale. Through this advance on his part, a helping hand will doubtless be held out to those who are occupied with the subject of stratification; for in this matter, the two advancing powers desire neither a "neutral zone" nor a "scientific frontier" to separate their field of action.

Another communication, full of promise as well of performance, should also not pass unnoticed. I allude to Professor Hughes' paper on his Induction Currents Balance, the application of which has already branched out and borne fruit in more than one direction. The extreme simplicity of the instruments, and their marvellous adaptation to the purposes for which they were intended, reflect the highest credit on their inventor.

Not unconnected with Mr. Crookes' researches, so far as they are directed towards the ultimate constitution of gases, are those of Professor Osborne Reynolds. In his remarkable paper, "On Certain Dimensional Properties of Matter in a Gaseous State," Professor Osborne Reynolds has established the fact of what he calls Thermal Transpiration, namely, that when two portions of the same gas are separated by a porous plug, the two surfaces of which are of different temperatures, the condition of equilibrium is no longer that the pressures of the gas on the two sides of the plug should be equal, but that the pressure on the hotter side should exceed that on the colder side by a certain quantity; and that if this is not the case, the gas will transpire through the plug until this condition is satisfied. Professor Reynolds connects this principle with Mr. Crookes' Radiometer experiments; and the various considerations which he brings to bear on the subject appear to have this idea throughout, that the dynamical similarity of two gaseous systems, bounded by solid surfaces, geometrically similar, depends on the ratio of the homologous lines of the system to a certain quantity, having the dimensions of a line, and which varies inversely as the density of the gas. Although dimensional considerations have been shown by previous writers to have an important bearing on the Theory of Gases, still his experimental results form an important contribution to our knowledge of the subject; and in his theoretical investigation he has attacked with vigour, and not without success, some elements of the problem avoided by his predecessors in this field of research.

Photography has of late years become so completely the handmaid of Physical Science, and has so constantly responded to the calls of its master, that its progress, however rapid and however effective, has come to be taken almost as a matter of course. But the step which Captain Abney appears now to have taken lies entirely out of the ordinary line

of advance. M. Becquerel, as is well known, succeeded some years ago in producing natural colours by the agency of light; one specimen of which, a solar spectrum, was given to me some three years since by M. Cornu, and which, through being carefully preserved in a tin box, retains all its chromatic features. Captain Abney has now taken up the subject again, and believes it probable that the colours obtained by this process may be preserved unchanged when exposed to ordinary daylight. His suggestion as to the physical causes of the colour is interesting, and leads us to hope that we may hear more upon the subject.

It was at one time thought that Science and practical life were essentially distinct, and that in their cares and their purposes, in their sorrows and their joys, neither intermeddled with the other. But in proportion as it has been gradually recognised that Science, and even Philosophy itself, is based upon experience, so has their distinction gradually faded from our view. And, among many other instances of forecasts in this direction by far-seeing men, I may adduce the dream of Babbage, that mathematical calculation might be reduced to mechanism, and that the data of many problems both of physics and of life might be handed over to an engine which would work out the results. That dream of his was as nearly realised as any dream of his ever could be realised; for the merit and the charm of the man himself, and the fascination which he threw about all his projects, were derived from the fact that, at whatever rate realisation followed, his ideas flew faster and faster still and outstripped them all. And if, in the machinery for complicated calculation designed by the brothers Thomson, much more has been actually achieved than ever before, it is not because this is a generation of more feeble folk, or because there has been any lack of elasticity of mind or of fertility in idea, but because their minds and their ideas have been more under control, and because these men have succeeded in that which their great predecessor always missed, namely, mastery over themselves.

The relations between the Mechanism of the Heavens and the History of the Earth have for some years past been the subject of speculation and of argument amongst many prominent men of science. And calculations have been made, tending more or less to confirm the views of the various writers on the subject. But so vast is the problem, so multifarious in its data, so complicated in its laws, that eager as we are to grasp anything solid or well founded in the inquiry, we are constantly left with a feeling that we may have clutched at a mere floating weed, when we hoped that we had laid hold of something firmly rooted. And on this account we may look with unusual satisfaction at the massive and philosophical research in which Mr. George Darwin is engaged, and of which we have already some substantial instalments. It consists in an investigation on the

precession and tides in a viscous spheroid, in which he applies his results to a discussion of the internal condition of the several planets, and of the evolution of the moon and other satellites from the primitive nebulæ.

I now pass on to mention briefly a few other subjects which, outside our immediate sphere of action, form substantial elements of the scientific progress of the past year, and among these I should first mention Meteorology, because the Council, to whom in this country the subject is entrusted, is nominated by the Royal Society.

The science of Meteorology has, during the last few years, attracted an increasing amount of attention, more perhaps from its close relation to the interests of all classes of the community, than from the definiteness or novelty of the scientific results to which it has led.

The second International Congress of Meteorology met at Rome in April of the present year, on the invitation of the Italian Government; an interval of nearly five years having elapsed since the date of the first Congress, which was held at Vienna in 1874. The main object of these meetings has been to introduce greater uniformity of method into the meteorological systems of Europe, without attempting, however, to pledge the different Governments to any definite engagements on the subject. With this view, a permanent International Committee has been constituted, to furnish a *point d'appui* for common action, and to facilitate, during the intervals between the Congresses, a personal interchange of opinions among the authorities directing the different systems.

A great number of valuable reports, on questions embracing nearly the whole field of Meteorology, were received by the Roman Congress. Among them, special mention may be made of the reports by M. Violle on Solar Radiation; by M. Pernet, on the Determination of the Fixed Points of Thermometers; and by Professor Everett, on Atmospheric Electricity.

A Conference of a semi-official character, which was originated by the Congress at Rome, was held at Hamburg in the beginning of October, for the purpose of taking into consideration a proposal, emanating from Lieutenant Weyprecht and Count Wilczek, to establish for one complete year a circle of meteorological observations round the Arctic regions of the globe. The Conference was attended by representatives of the Meteorological Institutes of Austria, France, Germany, Russia, and the Scandinavian countries; and it was agreed that an effort should be made to establish the proposed circumpolar system of meteorological and physical observations in the year 1881. Some part at all events of this plan will, in all probability, be carried into effect, as the Canadian Government has intimated its readiness to co-operate in it, and as Count Wilczek has offered himself to defray the cost of an Observatory at Nova Zembla.

The unfavourable weather which has characterised the present year, has given an additional impulse to the study of English Meteorology; and our own Meteorological Office has ventured to resume the issue of Daily Forecasts of Weather, after they had been discontinued for nearly thirteen years. The attempt has been made under somewhat adverse circumstances, as the uncertainty of the weather of 1879 has even surpassed the proverbial fickleness of the British climate. But the experiment has been liberally supported by the Press, and has been watched with great interest by the public generally. Of its success or failure it would as yet be premature to speak, but it deserves recognition as a serious attempt to apply to one of the most uncertain of the sciences the severe test of prediction.

Transit of Venus (1874) Reductions.—The calculations of every kind relating to the five British Expeditions were some time ago concluded. The mass of them refers to the determinations of terrestrial longitudes and the discussion of the measurements of photographs. The preparation of the Astronomical (as distinguished from the Photographic) Observations and Results for Printing is complete for the expeditions to the Hawaiian Islands, Rodriguez Island, and New Zealand. For the other two expeditions much remains to be done—chiefly, however, mere copying. The employment of the staff upon the measurement, re-measurement, and discussion of the photographs, was the most serious cause of the delay that has occurred in presenting to the public the astronomical results. Those who are now engaged upon this subject entertain serious doubts as to whether any result of value, as regards the special object of the expeditions, is to be expected from the photographs, owing to a want of sharpness in the images. Some of the photographs taken by the Russian Astronomers* have been measured, but not very successfully. The telescopic observations of the internal contacts, at many stations in India and Australia, as well as at those occupied by the Government Observers, in all thirty-one observations of the *Ingress* and forty-eight of the *Egress*, have yielded a value of the mean solar parallax of $8''{\cdot}85 \pm 0''{\cdot}03$.†

The Greenwich Nine-Year Catalogue of 2,263 stars, with discussions of the systematic errors of R.A.'s and N.P.D.'s has been published.

The complete reduction of the measures of about 1,900 photographs of the sun, taken at Greenwich from 1873 up to the present time, affords data immediately available for determining the distribution of spots and faculæ on the sun's surface, and the position of its axis of rotation. The photographs now regularly taken at Greenwich may be considered a continuation of the Kew series.

The reduction of the Greenwich magnetic observations, 1841—

* At Haven Possuit.

† "Monthly Notices, R.A.S.," xxxviii, p. 455.

1877, has afforded an opportunity for testing the connexion (first suggested by Sir E. Sabine and afterwards investigated as regards the element of declination by Professor R. Wolf) between magnetic variations and sun-spots. Mr. Ellis has compared the diurnal ranges of magnetic declination and horizontal force with Professor Wolf's curve of sun-spot frequency, with the result that not only is there a general correspondence in the two sets of phenomena, but the minor irregularities of the sun-spot curve are reproduced in the curves of diurnal magnetic range for both elements, and further that the well-marked annual inequality in the latter is itself variable, being greatest at the time of maximum of sun spots and least at that of minimum.

The spectroscopic determination of the motions of stars in the line of sight by the displacement of lines in their spectra, has been recently extended at Greenwich to stars of the third and fourth magnitude, raising the number of stars available for the application of this method to 200 or 300. Up to the present time, the motions of 63 stars have been thus determined at Greenwich. Mr. Seabroke, at Rugby, has also applied the method to 28 stars, and his results, though presenting some discordances *inter se*, generally support those obtained by Mr. Huggins and by the Greenwich observers.*

A new determination of the Figure of the Earth, in which the results of recent measurements of the Indian arc of 24° are included, has been made by Colonel Clarke.†

A connexion between the Sun's Outer Corona and Meteor Streams has been suggested by several observers, Professor Cleveland Abbé, Mr. F. C. Penrose, and others,‡ of the Solar Eclipse of 1878.

From a discussion of the distribution of aphelia and inclinations of cometary orbits, Professor H. A. Newton has come to the conclusion that comets have come to us from the stellar spaces (in accordance with Laplace's hypothesis), and that (with the possible exception of the comets of short period) they have not originated within the solar system as Kant supposed.§

Mr. Gill has deduced from his extensive series of Heliometer Observations of Mars at Ascension, the value $8''{\cdot}78 \pm 0''{\cdot}015$ for the Sun's Mean Equat. Hor. Parallax.‖ This agrees nearly with the first published result of the British Transit of Venus Expeditions, and also with the result of Cornu's determination of the velocity of light combined with Struve's constant of aberration.

* "Monthly Notices," 1879, June. 1879, October 10.
† "Phil. Mag.," 1878, August.
‡ "Monthly Notices," 1878, November.
§ "Amer. Journ.," 1878, September.
‖ "Monthly Notices," 1879, June.

The publication of Lohrmann and Schmidt's Maps of the Moon forms an important contribution to Selenography.

M. Flammarion has published a valuable Catalogue of Binary Stars, giving all available observations (to the number of 14,000) of these objects, with the elements of their orbits, so as to present a complete summary of their history.

Among the more recent Institutions for the promotion of Science, mention should be made of the Observatories for the study of Astronomical Physics in France and Germany.

The elder of these in point of date, namely that in France, although founded in 1876, has only quite recently been permanently established in the Chateau at Meudon. The Director has ever since the foundation been principally occupied with solar photography, and he has established the fact that photography is capable of revealing phenomena which must necessarily escape ordinary telescopic observation. In particular, he has succeeded in photographing the granulations previously seen on the solar disk by other astronomers; and by means of his magnificent pictures, one of which he has recently presented to the Society, he has established the existence of what he terms the *Réseau photospherique;* in other words, that the surface of the sun is divided into regions of calm and of activity. His present researches will doubtless enrich our knowledge with many new facts.

The German Observatory has been placed at Potsdam. In the selection of the locality, 320 feet above the sea, with a good horizon, and free from the smoke and vibration of the city, and in the furnishing of the Observatory, the German Government has done everything that could be wished. And it may confidently be hoped that in the hands of Professor Vogel and his colleagues much good work will be there done.

The ramifications of Science are now so various in direction, so comprehensive in grasp, and so immediate in their applications, that they have of late years given rise to the establishment of special Institutes and Societies for the study and encouragement of their respective subjects. Among these there may be mentioned the Institution of Telegraph Engineers, the Iron and Steel Institute, and others.

At the meetings of these bodies it frequently happens that subjects which may have, in their primary form of a scientific discovery, been laid before the Royal Society, are brought up again and discussed from a different point of view, *e.g.*, in their relation to other subjects, or in their application to the special purposes of life. For instance, in the "Proceedings of the Institution of Telegraph Engineers" we find the following:—

1. Wires which run parallel to each other for a long distance are very much troubled by induction between wire and wire. The effect

on telegraphs is to diminish speed of working, and on telephones to render speaking impossible. Hughes has studied these effects in his own peculiar way, and has shown how, when the number of wires is limited and the working confined to one direction, they can be entirely eliminated.

2. Mr. Cowper has shown how, by producing the two rectangular components of a plane curve at a distance, it is possible to reproduce handwriting. His instrument works very effectively, but is still perhaps in an early stage of its development.

3. The application of the Duplex system to long cables has been practically and very efficiently carried out by Mr. Stearns on the Atlantic Cable, and by Mr. Muirhead on the long cables of the Eastern Telegraph Company in the Mediterranean and Indian Ocean.

4. The Electric Light is still on its trial for general street illumination in Paris and London; but it has been recently very effectively employed in surveying and sounding the Mediterranean by night, and in the operation of laying and repairing cables by the steam-ship "Dacia," belonging to the Silvertown Company. By its aid ninety-six soundings, made with Sir William Thomson's steel wire apparatus, in water averaging 1,500 fathoms depth, were taken in seven days and nights, bottom in every case being brought up.

5. Several improvements in Telephones have been brought out, notably Gower's, in Paris, and Edison's loud-sounding arrangement, and the instruments are gradually finding their way into practical use.

6. Experiments are being made both in England and in France in the transmission of mechanical power by electric currents; and we may hope for valuable results from this field of inquiry.

The Iron and Steel Institute was founded in 1869 under the presidency of the Duke of Devonshire. The Society consists of proprietors and managers of iron and steel works, of metallurgical chemists, geologists, and engineers. The number of its members has grown gradually from 140 at its commencement to its present limit, 1,031, of whom 116 are foreigners residing abroad, and comprising the leading metallurgists of America as well as of the continent of Europe. The Association meets twice a year, once (during the spring) in London, and once (in the autumn) by invitation at one of the great industrial centres in England or abroad. In 1873 it met at Liège, in 1878 at Paris, and in 1880 it will meet at Düsseldorf. Each meeting lasts from two to four days, which are spent partly in reading and discussing papers, and partly in visiting works of interest.

The results of the Association have been of considerable utility: (1) by bringing the scientific metallurgist into contact with the man of practical experience, thereby extending the appreciation, and in many

instances even the actual cultivation, of Science; (2) by the annual publication of a very valuable record of the progress made in this important branch of applied Science; (3) by acting as a kind of international tribunal on the merits or demerits of new processes, in a manner calculated both to stimulate and to guide further undertakings.

Telegraph Conference.—When Telegraphy began to assume an international character, great inconvenience arose from want of uniformity in the apparatus used, and in the service regulations adopted by different European countries.

Although the construction of the line (whether submarine, underground, or suspended) and the details of construction of the telegraphic apparatus employed may fairly be left in the hands of each country or telegraphic administration, it was soon found to be necessary that a certain standard of efficiency should be insisted upon. Agreement was moreover requisite in regard to the general character of the instruments to be used in international telegraphy; for instance, whether or not acoustic signals were to be admissible, whether an indelible record should be insisted upon, whether a type-printer, or an autographic instrument should be used, or a recording instrument representing the letters by dots and dashes, in which case a uniform code would be a matter of necessity. Beside these points there remained the subject of the tariff to be charged in each country for transit and for terminal messages, involving delicate questions, which could be settled only by mutual concessions and general arrangements.

These considerations led the French Government, in 1866, to invite those European Governments who had already taken the administration of the telegraphs in their own hands, to meet at Paris, in order to agree if possible on certain resolutions, subject to ratification by the respective Governments. England was not represented at this first Conference, because the telegraphs were not yet a Government Department, and it was not thought advisable to invite telegraph companies to take part in the proceedings.

The result of the first deliberations went beyond the mere settlement of pressing questions, and it was decided to hold periodical Conferences at the different capitals of Europe, and to institute a central International "Bureau" (to be located at Berne) to see to the proper application of the resolutions of the Conferences, and to act as an International Court of Appeal, regarding questions of difference in telegraphic matters arising between the contracting countries.

The second Conference took place at Vienna in 1869, in which several countries, including Turkey, Persia, and British India, took part for the first time. On this occasion it was resolved that the leading telegraph companies should be admitted to the discussions, but should not be allowed to vote; the power of voting being confined

to the Governments, and each country having one vote only, irrespective of its size and importance.

The third Conference was held at Rome in December, 1871, when the British Government (Post Office) was for the first time represented, and also the Indo-European and Cable Companies, which latter are bound by the resolutions of the Conference only in matters of international administration, and of tariff in cases where lines join two countries belonging to the Union.

The fourth Conference met at St. Petersburg in 1876, and the fifth at London in 1879. At the latter meeting difficult questions of tariff arose, which protracted the sittings over nearly two months. At previous Conferences it had already been decided to treat each country as a unit, its particular charge being the same, whether a message had only to cross the frontier, or had to pass over long distances into the interior. At the London Conference it was proposed by Germany to adopt a uniform rate per international message for the whole of Europe, analogous to the uniform postal rate for letters, while France went further and urged the adoption of a universal *rate per word.* Neither of these measures, however, was adopted, but a compromise was effected: and after the 1st April, 1880, each word will be charged at a specified rate for each country, five words being added to each message to cover time spent in advising, commencing, and finishing it.

The next Conference will meet at Berlin in 1884.

All European countries, as well as Brazil, Persia, India, and Japan, belong to the Conference, and are bound by its decisions.

Any of twelve modern languages using the Roman character may be employed; Russian, Greek, Turkish, being thus excluded from use.

It would be difficult to over estimate the beneficial results produced by these Conferences in stimulating and perfecting international communication.

Material is not wanting to extend these notices of scientific activity over a wider range; for our foreign neighbours are always ready to furnish us with information of whatever is new among them either in matter or in method. But a mere enumeration of these things, however interesting in themselves, would form but a dull chapter, unless it should issue in a judicial estimate of results, or a suggestive comparison of national arrangements for the promotion of science. On such an enterprise I am by no means prepared to adventure myself; and even if I should be ever led to make the attempt on some special topic, or some particular aspect of the question, I must, at all events, defer it to a future occasion.

On the motion of Mr. Francis Galton, seconded by Sir Joseph Fayrer, it was resolved:—"That the thanks of the Society be re-

turned to the President for his Address, and that he be requested to allow it to be printed."

The President then proceeded to the presentation of the Medals :—

The Copley Medal has been awarded to Rudolph Julius Emmanuel Clausius, Foreign Member of the Royal Society, for his investigations in the Mechanical Theory of Heat.

The Mechanical Theory of Heat as at present understood and taught has been so essentially a matter of growth, that it would be difficult to assign to each investigator the precise part which he has taken in its establishment. It will, however, be admitted by all, that the researches of Clausius rank high among those which have mainly contributed to its development. These researches extend over a period of thirty years, and embrace important applications of the theory not only to the steam engine, but to the sciences of electricity and magnetism.

Even to enumerate those who have contributed to one branch of the subject, viz., the Kinetic Theory of Gases, would be beyond my present purpose and powers; but as Clausius himself states, both Daniel and John Bernoulli* wrote on the subject. And, even, to go back to earlier times, Lucretius† threw out the idea; while Gassendi, and our own Boyle, appear to have entertained it. Within our own recollection, Joule, Meyer, Kroning, Clerk Maxwell, and others have made invaluable contributions to this branch, as well as to the general subject of the Mechanical Theory of Heat. But however great the value of these contributions, it may safely be stated that the name of Clausius will always be associated with the development of earlier ideas into a real scientific theory.

The Davy Medal has been awarded to P. E. Lecoq de Boisbaudran. The discovery of the metal gallium is remarkable for having filled a gap which had been previously pointed out in the series of known elements. Mendelejeff had already shown that a metal might probably exist, intermediate in its properties between aluminium and indium, before Boisbaudran's laborious spectroscopic and chemical investigation of numerous varieties of blende led him to the discovery and isolation of such a metal.

The separation of the minute traces of gallium compounds from blende is an operation presenting unusual difficulty, owing to the circumstance that compounds of gallium are carried down by various precipitates from solutions which are incapable by themselves of depositing those compounds.

* In the 10th section of his "Hydrodynamics."

† "De rerum Naturâ," lib. ii, 111—140.

A Royal Medal has been awarded to A. C. Ramsay, F.R.S. Professor Ramsay has been for a period of nearly forty years connected with the Geological Survey of Great Britain, and during by far the greater part of that time either as Director or Director-General of the Survey. During this long period in addition to his official labours in advancing our knowledge of the geology of this country, he has published works on the "Geology of Arran," "The Geology of North Wales," "The Old Glaciers of North Wales and Switzerland," and "The Physical Geology and Geography of Great Britain," now in its fifth edition. His papers in the "Quarterly Journal of the Geological Society," and elsewhere, are numerous and important, especially those on theoretical questions in physical geology, such, for instance, as "The Glacial Origin of Lake Basins," "The Fresh-water Formation of the Older Red Rocks," and "The History of the Valley of the Rhine, and other Valleys of Erosion." There are, indeed, among living geologists few who can claim to have done more to extend our knowledge in the important fields of geology and physical geography.

A Royal Medal has been awarded to W. H. Perkin, F.R.S. Mr. William Perkin has been, during more than twenty years, one of the most industrious and successful investigators of Organic Chemistry.

Mr. Perkin is the originator of one of the most important branches of chemical industry, that of the manufacture of dyes from coal-tar derivatives.

Forty-three years ago the production of a violet-blue colour by the addition of chloride of lime to oil obtained from coal-tar was first noticed, and this having afterwards been ascertained to be due to the existence of the organic base known as aniline, the production of the colouration was for many years used as a very delicate test for that substance. The violet colour in question, which was soon afterwards also produced by other oxidising agents, appeared, however, to be quite fugitive, and the possibility of fixing and obtaining in a state of purity the aniline product which gave rise to it, appears not to have occurred to chemists until Mr. Perkin successfully grappled with the subject in 1856, and produced the beautiful colouring matter known as aniline violet, or mauve, the production of which, on a large scale, by Mr. Perkin, laid the foundation of the coal-tar colour industry.

His more recent researches on anthracene derivatives, especially on artificial alizarine, the colouring matter identical with that obtained from madder, rank among the most important work, and some of them have greatly contributed to the successful manufacture of alizarine in this country, whereby we have been rendered independent of the importation of madder.

Among the very numerous researches of purely scientific interest

which Mr. Perkin has published, a series on the hydrides of salicyl and their derivatives, may be specially referred to; but among the most prominent of his admirable investigations are those resulting in the synthesis of coumarin, the odoriferous principle of the tonquin bean and the sweet scented woodruff, and of its homologues.

The artificial production of glycocol and of tartaric acid by Mr. Perkin conjointly with Mr. Duppa, afford other admirable examples of synthetical research, which excited very great interest among chemists at the time of their publication.

It is seldom that an investigator of organic chemistry has extended his researches over so wide a range as is the case with Mr. Perkin, and his work has always commanded the admiration of chemists for its accuracy and completeness, and for the originality of its conception.

The Statutes relating to the election of Council and Officers were then read, and Prof. François de Chaumont and Dr. Gladstone having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year :—

President.—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Professor Alexander W. Williamson, Ph.D., LL.D.

Other Members of the Council.—George Busk, V.P.L.S.; Professor Arthur Cayley, LL.D.; Major-General Henry Clerk, R.A.; Edwin Dunkin, F.R.A.S.; Augustus G. Vernon Harcourt, F.C.S.; Sir Joseph Dalton Hooker, C.B., K.C.S.I.; John Whitaker Hulke, F.R.C.S.; Lieut.-General Sir J. Henry Lefroy, C.B.; William Newmarch, Inst. Fr. Corr.; Professor Alfred Newton, M.A., V.P.Z.S.; Professor William Odling, M.B., V.P.C.S.; Sir James Paget, Bart., D.C.L.; William Henry Perkin, Sec. C.S.; Charles William Siemens, D.C.L.; John Simon, C.B., D.C.L.; Professor John Tyndall, D.C.L., LL.D.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1878 ..	4	44	253	248		549
Since Elected ..		+ 6	+ 4	+ 1	+ 11	+ 22
Since Deceased..		— 2	— 16	— 13		— 31
December 1, 1879.	4	48	241	236	11	540

Statement of Receipts and Expenditure from November 28, 1878, to November 19, 1879.

	£	s.	d.
Annual Contributions, 242 at £4 ..... £968			
„ 11 at £3 33	1,001	0	0
Admission Fees	10	0	0
Compositions	240	0	0
Rents	300	13	1
Dividends (exclusive of Trust Funds)	1,143	16	5
„ on Jodrell Fund	152	4	10
Interest on Mortgage Loan	587	10	0
Sale of Transactions and Proceedings	871	12	7
Interest on Deposits	12	14	4
Bequest of the late Sir W. C. Trevelyan	1,500	0	0
„ „ Sydney Ellis, Esq.	1,000	0	0
Donation from G. Palmer, Esq., M.P.	200	0	0
Sale of land at Acton	600	0	0
Balances from 1878	916	4	7
	£8,535	15	10

	£	s.	d.	£	s.	d.
Salaries and Wages				1,146	3	0
Illustrations and Paper for Report of Naturalists (Transit-of-Venus Expedition)				217	9	8
The Scientific Catalogue				123	19	0
The Library Catalogue				112	0	0
Books for the Library				214	0	5
Binding ditto				67	4	1
Printing Transactions, Part II. 1878, and Separate Copies to Authors and Publisher	223	16	2			
Ditto Proceedings, Nos. 190–197	380	7	5			
Ditto Miscellaneous	62	19	10	1,409	7	10
Paper for Transactions and Proceedings	159	13	6			
Binding ditto	61	11	2			
Engraving and Lithography	520	19	9			
Soirée and Reception Expenses				77	19	6
Coal, Lighting, &c.	108	10	9			
Office Expenses	17	11	1			
House Expenses	95	8	0			
Tea Expenses	19	18	0			
Fire Insurance	41	15	0	392	15	3
Taxes	35	3	9			
Advertising	22	4	6			
Postage, Parcels, and Petty Charges	31	5	10			
Miscellaneous Expenses	20	18	4			
Mablethorpe Schools, Donation				2	2	0
Deposited at Bankers				1,800	0	0
Purchase of Land at Acton				228	0	0
Law Charges				4	13	10
Purchase of £1,396 G.N.R. 4 per Cent. Debentures				1,499	11	0
				£7,295	5	7

Trust Funds.

	£	s.	d.		£	s.	d.	
Donation Fund Dividends	186	4	4					
Handley Fund „	177	12	10					
Rumford Fund „	68	4	8					
Wintringham Fund „	35	5	0	512 9 3				
Copley Medal Fund „	9	17	6					
Davy Medal Fund „	32	6	2					
Croonian Lecture Fund—Rent from College of Physicians	2	18	9					
					Donation Fund	100	0	0
					Handley Fund—Purchase of £149 5*s.* 4*d.* Reduced 3 per Cents	141	1	2
					Rumford Medal Fund	137	7	1
					Davy Medal Fund	63	2	3
					Wintringham Fund	35	8	0
					Copley Medal Fund	4	15	1
					Bakerian Lecture	4	0	0
					Croonian Lecture	2	19	3
					Balance at Bank			
					Balances on hand, Catalogue Account	32	2	9
					, „ Petty Cash	9	11	11
			£9,048 5 1					£9,048 5 1

488 12 10

1,222 12 0

41 14 8

JOHN EVANS,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
Estate at Acton, Middlesex (about 33 acres), £161 12*s.* 10*d.* per annum.
Fee Farm near Lewes, Sussex, rent £19 4*s.* per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One-fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £508 17*s.* 3*d.* in 1878-79).
£20,047 7*s.* 9*d.* Reduced 3 per Cent. Annuities.
£15,000 Mortgage Loan, 4 per Cent.
£16,588 6*s.* 2*d.* Consolidated Bank Annuities.
£403 9*s.* 8*d.* New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
£11,511 6*s.* New Threes { £6,328 11s. 2*d.* Scientific Relief Fund. 5,182 14*s.* 10*d.* Jodrell Fund.
£667 5*s.* 6*d.* India Fours.
£660 Madras Guaranteed 5 per Cent. Railway Stock.--Davy Medal Fund.
£10,000 Italian Irrigation Bonds.—The Gassiot Trust.
£1,396 Great Northern Railway 4 per Cent. Debentures—The Trevelyan Bequest.
£985 4*s.* 5*d.* Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.
£6,000 London and North Western Railway 4 per Cent. Debentures. } ,, ,,
Two Hundred Shares in the Whitworth Land Company, Limited. } ,, ,,
£240 Deposited at Bankers. ,, ,,
£1,800 ,, ,, on General Account.

Trust Funds. 1879.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	6,328	11	2
Metropolitan 3½ Consols	100	0	0
	£6,428	11	2

Dr. *Cr.*

	£	s.	d.		£	s.	d.
To Balance	169	11	7	By Grants	260	0	7
„ Dividends	189	8	0	„ Balance	98	19	0
	£358	19	7		£358	19	7

Donation Fund.

£6,339 0*s.* 1*d.* Consols.

The Trevelyan Bequest.

£1,396 Great Northern Railway 4 per Cent. Debentures.

	£	s.	d.		£	s.	d.
To Balance	133	1	0	By Grants	100	0	0
„ Dividends	186	4	4	„ Balance	219	5	4
	£319	5	4		£319	5	4

Rumford Fund.

£2,322 19*s.* Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance, 1878	137	7	1	By Medals	59	8	1
„ Dividends, 1879	68	4	8	„ A. Cornu	77	19	0
				„ Balance	68	4	8
	£205	11	9		£205	11	9

Bakerian and Copley Medal Fund.

£403 9*s.* 8*d.* New 2½ per Cent.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	73	19	1	By Gold Medal	4	15	1
„ Dividends	9	17	6	„ Bakerian Lecture	4	0	0
				„ Balance	75	1	6
	£83	16	7		£83	16	7

Wintringham Fund.

£1,200 Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance, 1878	35	8	0	By Payment to Foundling Hospital, 1879	35	8	0
„ Dividends, 1879	35	5	0	„ Balance	35	5	0
	£70	13	0		£70	13	0

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To Balance, 1878	2	19	3	By Croonian Lecture	2	19	3
„ One-fifth of Rent of Estate at Lambeth Hill, payable by the College of Physicians	2	18	9	„ Balance	2	18	9
	£5	18	0		£5	18	0

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	170	7	3	By Reverse Die	2	11	0
„ Dividends	32	6	2	„ Gold Medals	60	11	3
				„ Balance	139	11	2
	£202	13	5		£202	13	5

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

	£	s.	d.		£	s.	d.
To Balance	291	0	0	By Payments to Kew Committee	495	14	0
„ Dividends	495	14	0	„ Balance	291	0	0
	£786	14	0		£786	14	0

The Jodrell Fund.

£5,182 14*s.* 10*d.* New 3 per Cent. Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1879	152	4	10	By Payment to Royal Society	152	4	10

The Handley Fund.

£6,047 7*s.* 9*d.* Reduced 3 per Cent. Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance, 1878	141	1	2	By Purchase of £149 5*s.* 4*d.* Reduced	141	1	2
„ Dividends, 1879	177	12	10	„ Balance	177	12	10
	£318	14	0		£318	14	0

Fee Reduction Fund. November 19th, 1879.

	£	*s.*	*d.*		£	*s.*	*d.*
To Subscriptions	7,111	5	0	By 200 Shares in the Whitworth Land Company, Limited	2,000	0	0
„ Sir Joseph Whitworth's Donation of 200 Shares in the Whitworth Land Company, Limited	2,000	0	0	„ Purchase of £985 4*s.* 5*d.* Metropolitan 3½ per Cent. Consols	1,000	0	0
„ Transferred from the Jodrell Fund	1,000	0	0	„ Purchase of £6,000 London and North-Western Railway Company's 4 per Cent. Debenture Stock	6,408	9	4
„ Interest on Deposits	32	14	1	„ Power of Attorney	0	11	6
„ Dividends on Stocks and Shares	261	8	3	„ Balance at Bank £756 6 6			
				„ Do. on Deposit 240 0 0			
					996	6	6
	£10,405	7	4		£10,405	7	4

Account of Grants from the Donation Fund in 1878–79.

Messrs. Morrison and Marsden, for a Research into the Solubility of Carbon and Silicon in Fused Metals, and the Form in which they crystallise out on cooling	£20	0	0
J. Snodgrass, for a Research into the Absorption of Gases by Solutions of Salts	50	0	0
Sir William Thomson, for the Construction of a Machine for solving Simultaneous Linear Equations	30	0	0
	£100	0	0

Account of the appropriation of the sum of £1,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the Advancement of Science (continued from Vol. XXVIII, p. 75.)

1879.

1. Dr. Stenhouse, for continuation of Researches into the History of Naphthaline and its Derivatives, and into Gardenine £100

2. W. Crookes, for continuation of Researches on the Viscosity of Gases at various Exhaustions, and on the Trajectory of Molecules 200

3. J. Glaisher, towards the expense of Printing the Factor Table of the Fourth Million 150

4. D. Gill, for obtaining assistance of Computors in finishing the Reductions of the Observations which he made on Mars, at Ascension, 1877 100

5. Capt. Abney, for continuing his Researches on the Spectra of Skylight, and his Investigations on Photographing the Ultra-red Portion of the Solar Spectrum 30

6. G. B. Buckton, for aid in Publishing a Monograph of the British Aphides, which may lead to an increased knowledge of the Group, and the Phenomena concerned in their Reproduction 75

7. G. E. Dobson, for Investigation of the Natural History of the Mammalian Order Insectivora, embracing—(1) Descriptions of all the known species; (2) their distribution in space and time; (3) the anatomy of the least known, most interesting, and most typical species 25

Carried forward..... £680

Brought forward.....	£680
8. Professor Leith Adams, for completion of the Description of the Remains of British Fossil Elephants	30
9. W. J. Sollas, for an Investigation into the Microscopic Structure of Fossil Sponges	25
10. W. T. Thiselton Dyer, for aid in the Preparation of an Illustrated Monograph of Cycadeæ	25
11. E. A. Schäfer, for Payment of an Assistant in continuing his Histological and Embryological Investigations	50
12. Baron Ettingshausen, for Expenses incurred in continuing the Preparation of his Monograph on the Eocene Flora of Great Britain	50
13. Professor A. H. Church, for the purpose of completing Researches in Plant Chemistry, and for extending the inquiry into new directions	40
14. E. J. Mills, for the further prosecution of Researches on Standard Industrial Curves	70
15. J. N. Langley, for a Research into the Secretion of the Salivary Glands, and on the Effect of Section and Stimulation of any one Nerve of the Frog on the Reflex-time of the rest	50
	£1020

Dr. *Cr.*

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance on hand, Nov. 30, 1878	1032	5	8	By Appropriations, as above.....	1020	0	0
Grant from Treasury, 1879	1000	0	0	Printing, Postage, and Advertising	4	2	6
Interest.....	14	6	9	Balance on hand, Dec. 1, 1879	1022	9	11
	2046	12	5		2046	12	5

Account of Appropriations from the Government Fund of £4,000 made by the Lords of the Committee of Council on Education, on the recommendation of the Council of the Royal Society.

Arnulph Mallock, for the cost of Construction of a Machine for Ruling Diffraction Gratings of large size	£200
G. Gore, for continuation of Researches into the Thermo-electric Properties of Liquids; the Determination of the Electromotive Force, and internal resistance of a Liquid Thermo-electric Battery, and to complete Tables of Electric Relations of Metals at different Temperatures	100
Sir Wm. Thomson, for completing a Tidal Harmonic Analyser	50
Sir Wm. Thomson, for continuation of Experiments on the Effects of Stress on Magnetization	50
J. Kerr, for continuation of Electro-optic and Magneto-optic Investigations	100
J. H. Poynting, for continuation of Experiments to determine the Mean Density of the Earth, by means of an Ordinary Balance	93
Rev. Dr. Haughton, for continuing his Discussion of Tidal Observations made in Arctic Seas	50
J. N. Lockyer, for continuation of Researches on the Solar Spectrum	200
A. Tribe, for Researches into the Distribution of the Radicals of Electrolytes on Metallic Conductors	50
W. C. Roberts, for continuation of Researches on the Passage of Metals and Alloys in a Molten State through Capillary Tubes	25
R. H. Ridout, to investigate more fully the Sound developed in Homogeneous Conductors by Electrical Disturbance	15
Professors Liveing and Dewar, for continuing their Researches into the Spectra of Elements and Compounds under various circumstances	100
E. Neison, for continuation of Computations in the Lunar Theory	75
Alex. Buchan, for the expense of collecting and discussing Data for Determination of the Hourly Means of the Barometer for each Month, from a large number of Stations, on Land and Sea over the Globe	100
W. E. Ayrton, for extending to other Gases the Investigations already made in Air (by Professor Perry and himself) on the Electro-motive Force of Contact of Solids and Liquids	100
Carried forward	£1308

Brought forward.....	£1308
J. B. Jordan, for the construction of a Glycerine Barometer, and for carrying on Observations thereon.....	30
W. Galloway, for continuing his Investigation of the Effect of Coal Dust in promoting the Explosion of a Mixture of Air and Fire Damp	80
H. Tomlinson, for a Research into the Effect of Strain and Pressure on Matter when under the Influences of the Forces of Heat, Electricity, Magnetism, and Chemical Affinity	100
J. E. H. Gordon, for continuation of Experimental Measurements of the Specific Inductive Capacity of Dielectrics.....	100
Rev. G. Henslow, for aid in Researches in Vegetable Physiology: On the Absorption of Dew and Rain, and on the Transpiration of Plants	50
B. T. Lowne, for further Investigation of the Structure and Functions of the Eyes of Invertebrates.....	40
Professor M. F. Heddle, for a Research connected with the Scientific Mineralogy and Geognosy of Scotland	100
W. A. Forbes, for an Examination of the Pterylosis of Parrots, with a view to a better Arrangement and Classification of the Genera of that Sub-order.....	7
Dr. C. Callaway, for a Research into the relations of the Pre-Cambrian and Cambrian Rocks of Shropshire, extension thereof into Scottish Highlands, with a view to the more accurate correlation of the subdivisions of the Lower Palæozoic Rocks	50
Professor W. K. Parker, for assistance in continuation of Researches on the Morphology of the Vertebrate Skeleton, and the Relations of the Nervous to the Skeletal Structure.....	300
H. T. Stainton, in aid of the Publication Fund of the Zoological Record Association.....	100
Professor H. A. Nicholson and R. Etheridge, junior, for continuance of aid in Examining the Fossils of the Silurian Deposits of Girvan, Ayrshire, and in publishing a Monograph upon them	60
Colonel Godwin-Austen (Brit. Assoc. Committee), for Investigation of the Natural History of the Island of Socotra	175
Rev. A. E. Eaton, to defray the cost of Printing and Publishing a Descriptive Monograph of the Ephemeridæ.....	75
H. Woodward, for continuation of Work on the Fossil Crustacea, especially with reference to the Trilobita and other extinct Forms, and their Publication by the Palæontographical Society	75
Dr. W. H. Gaskell, for continuation of Researches upon Vasomotor Action.....	25
Carried forward.....	£2675

Brought forward	£2675
C. Lapworth, for assistance in work among the Silurian Rocks of South Scotland, and in special Studies of the British Graptolites	50
Professor H. G. Seeley, for continuation of Investigation into the Structure, Affinities, and Classification of the Extinct Reptilia and allied Animals	80
Professor W. Stirling, for Investigations into the Histology and Physiology of the Nervous Apparatus of the Lungs	25
Dr. W. R. Hodgkinson, for continuation of Researches on the Action of Chlorine and of dry Ammonia on Hydro-cinnamein, and other Investigations	175
C. G. Williams, for investigation of the Chemical History and Physiological Action of β Lutidine; and for continuation of Researches on Emeralds and Beryls	50
W. N. Hartley, for continuation of Researches on the Action of Organic Substances on the Ultra-violet Rays of the Spectrum	150
Professor Schorlemmer, for continuation of Researches into (1) Aurin; (2) the normal Paraffins; (3) Suberone	150
J. H. Collins, for continuation of Chemical, Mineralogical, Microscopical, and Stratigraphic Observations on and Investigations of the Rocks of Cornwall	30
E. A. Letts, for Experiments with Hæmat-oxyline, to determine its constitution, and, if possible, to obtain it by Synthesis; and for Experiments on the Action of Sodium on Hydro-chlorate of Turpentine and others	80
G. C. Robinson, for the Separation and Examination of Bases of High Boiling Points contained as Sulphates in the "Vitriol Tar" of the Paraffin Oil Manufacture, and the Determination of Composition and Properties of higher and, at present, unknown members of the Leucoline Series of Bases	15
Dr. C. R. A. Wright, for continuation of Researches on certain points in Chemical Dynamics, and on some of the less known Alkaloïds; and for a Research on the Determination of Chemical Affinity by means of Electrical Measurements	200
P. P. Bedson and W. C. Williams, for an Investigation into the Specific Refractive Powers of Benzene Derivatives, and of the Derivatives of other Aromatic Hydro-carbons	50
Dr. Armstrong, for continuation of Observations on the Phenols, and for Experiments on the Action of Reducing Agents on Nitric Acid	150
	3880
Administrative expenses	75
	£3955

Report of the Kew Committee for the Year ending October 31, 1879.

The Kew Committee has been increased by one new member, Lieutenant-General Sir J. H. Lefroy, K.C.M.G., during the past year, and is now constituted as follows:—

General Sir E. Sabine, K.C.B., *Chairman.*

Mr. De La Rue, *Vice-Chairman.*	Vice-Adm. Sir G. H. Richards.
Prof. W. G. Adams.	The Earl of Rosse.
Capt. F. Evans, C.B.	Mr. R. H. Scott.
Prof. G. C. Foster.	Lieut.-General W. J. Smythe.
Mr. F. Galton.	Lieut.-Gen. R. Strachey, C.S.I.
Lieut.-Gen. Sir J. H. Lefroy, K.C.M.G.	Mr. E. Walker.

Magnetic Work.—No change has been made in the Magnetographs, which have worked continuously during the year. The curves obtained have been, as a rule, devoid of interest, no disturbance of note having been registered for some time.

Owing to wear, the vertical force cylinder bearing has become loose in its seat, and it will be necessary shortly to make a new fitting for it.

In consequence of the illness of some of the members of the Sub-Committee mentioned in the last report, as appointed to consider the best means of utilising the accumulated records of the Magnetographs, no decision has yet been arrived at in the matter.

At the request of Professor Balfour Stewart the absolute diurnal range of the declination needle has been determined for every day for the period 1858 to 1873.

The scale values of all the instruments were re-determined in January, in accordance with the usual practice.

The monthly observations with the absolute instruments have been made regularly by Mr. Figg, and the results are given in the tables forming Appendix I of this report.

The magnetic instruments have been studied and a knowledge of their manipulation obtained by Mr. D. Gill, the Astronomer Royal at the Cape of Good Hope, and Mr. H. I. Bell, the recently appointed Assistant to the Royal Alfred Observatory, Mauritius.

Information on matters relating to terrestrial magnetism and various data have been supplied to the Hydrographic Office, Dr. E. Van Ryckevorsel, Dr. Atkinson, Dr. Thorpe, Mr. Elliott, Mr. Fleming, and Mr. Gordon.

To the last-named gentleman the Committee also lent illustrations of the magnetographs and other magnetic instruments employed at Kew for use in a work he has in preparation upon Electricity and Magnetism.

The following is a summary of the number of magnetic observations made during the year:—

Determinations of Horizontal Intensity	29
,, Dip	123
,, Absolute Declination	25

Meteorological Work.—The several self-recording instruments for the continuous registration respectively of atmospheric pressure, temperature, humidity, wind (direction and velocity), and rain have been maintained in regular operation under the care of Mr. T. W. Baker, assisted by Mr. J. Hillier.

The standard thermometer (No. 379) for eye observations of the temperature of evaporation was accidentally broken in September whilst re-covering the thermograph tube with new muslin. A spare thermometer (No. 473) of the same construction was at once set up in its place, so that no loss took place in the registrations.

The standard eye observations made five times daily, for the control of the automatic records, have been duly registered through the year, together with the daily observations at 0 h. 45 m. P.M. in connexion with the Washington synchronous system.

An additional daily observation has been made since July 1st at 6 h. 45 m. P.M., to be used in a second synchronous system organised by M. Mascart, Directeur du Bureau Central Météorologique, Paris.

The tabulation of the meteorological traces has been regularly carried on by Mr. Hawkesworth, and copies of these, as well as of the eye observations, with notes of weather and cloud, have been transmitted weekly to the Meteorological Office.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of standard barometer	2270
,, dry and wet thermometers.....	5818
,, maximum and minimum thermometers	976
,, radiation thermometers	1670
,, rain and evaporation gauges	830
Cloud and weather observations	2607

Measurements of barograph curves		9202
,,	dry bulb thermograph curves ..	9202
,,	wet bulb thermograph curves ..	9119
,,	wind (direction and velocity)..	18492
,,	rainfall curves	8760
,,	sunshine traces.....	2393

Instruction in the use of meteorological instruments has been afforded to the Rev. Clement Ley, Inspector of English Observing Stations, and Mr. H. I. Bell.

In compliance with a request made by the Meteorological Council to the Kew Committee, the Observatories at Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst, and Valencia, have been visited and their instruments inspected by Mr. Whipple during his vacation.

Mr. Baker, also in consequence of a request of the Meteorological Council, visited Stonyhurst Observatory in January last, and re-adjusted the thermograph, which had accidentally been deranged during the frost.

With the concurrence of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to and published by the "Times,"* "Illustrated London News," "Mid-Surrey Times," and "Torquay Directory," and meteorological data have been supplied amongst others to the editor of "Symons' Monthly Meteorological Magazine," Professor B. Stewart, Dr. Rowland, Mr. Greaves, C.E., and the Institute of Mining Engineers.

Copies of selected photographic curves have been made and supplied to M. Mascart.

Electrograph.—This instrument has been in continuous action through the year under the care of Mr. Constable.

It was found necessary in August to dismount and clean the instrument in order to restore its power of keeping a full charge of electricity, which had become somewhat impaired.

On consideration it was deemed inadvisable to alter the bifilar suspension as suggested in last year's report, but the Mascart insulating stands were fixed for the support of the water-dropping cistern, and the action of the instrument has been greatly improved by their adoption, so that strong charges of electricity are now registered even in the dampest weather.

Some trouble was experienced in maintaining a constant flow of water from the jet during the severe frosts of last winter, but it was not considered necessary to make special arrangements for heating the

* A detailed investigation was made in the month of January into the accuracy of the weekly meteorological records published in "The Times." The result on the whole was very favourable, the principal difference being found in the rainfall, which is usually given in excess of the true amount.

water in the cistern, a spare jet kept in readiness being screwed on when the other was observed to be frozen.

Photo-heliograph.—The re-examination of the measurements of the Kew sun-pictures, as noticed in the last report, was completed in January, and the preliminary reductions of $\frac{r}{R}$ have been steadily carried on throughout the year by Mr. Whipple, assisted by Mr. McLaughlin, who has been temporarily engaged for this purpose. It is expected that the above reductions will be completed by the end of the present year, or early in 1880.

Mr. Marth is still engaged on the reduction to heliocentric elements of the pictures from January, 1864, to April, 1870, inclusive.

All these operations have been conducted under the direction and at the expense of Mr. De La Rue.

The eye observations of the sun, after the method of Hofrath Schwabe, as described in the report for 1872, have been made on 151 days, in order to maintain for the present the continuity of the Kew records of sun-spots. The sun's surface was observed to be free from spots on 113 of those days.

Transit Observations.—Seventy-eight observations have been made of sun-transits for the purpose of obtaining correct local time at the Observatory. Eighty-six clock and chronometer comparisons have also been made. This work is performed by Mr. Constable.

Extra Observations.—The four solar-radiation thermometers and one terrestrial-radiation were observed daily until the 3rd of August, when all, with the exception of one black bulb thermometer, were completely destroyed by the great hailstorm which occurred on that date. Since then only one, the remaining black bulb, and a new terrestrial radiation thermometer have been daily observed.

The Campbell sunshine recorder, described in the Report for 1875, continues in action, and the improved form of the instrument, giving a separate record for every day of the duration of sunshine, has been regularly worked throughout the year, and its curves tabulated.

Two papers based upon these records have been read by the Superintendent before the Meteorological Society, and published in their Quarterly Journal. The first, "On the Relation between the Duration of Sunshine, the amount of Solar Radiation, and the Temperature indicated by the Black Bulb Thermometer," printed in vol. v, p. 142, shows that these three phenomena are intimately connected with each other, the curves resulting from the plotting down of the daily values given by the three instruments rising and falling together almost all through the year.

The second paper, published in vol. v, p. 213, on "The Duration of Sunshine, Extent of Sky clouded, and the Height of the Barometer," proves that at the Kew Observatory the amount of cloud varies

inversely as the barometric pressure between the limits of 29·0 and 30·3 inches, the variation being most rapid between 29·8 and 30·1 inches. Also that above 30·3 inches cloud increases with increasing pressure, attaining the mean about 30·5 inches, and rising above it at 30·6 inches.

Various experiments have been made with the sunshine-recorder from time to time with the view of improving its efficiency, and new instruments with certain modifications of the Kew pattern have been submitted for approval by Messrs. Beck and Mr. Casella.

Wind Component Integrator.—The numerous adjustments and electrical contacts, altogether 190 in number, of this instrument, render it particularly subject to derangement in this country, the damp atmosphere causing frequent failures in the electrical actions, and the high winds constantly shaking the screws loose. The continual necessity for readjustment causing a greater demand upon the time of the Observatory staff than the Committee feel themselves justified in bestowing upon it, they have reluctantly decided upon dismounting it, and it will shortly be returned to South Kensington, to be again set up in the Loan Collection of scientific apparatus.

Photo-nephoscope.—At the request of the Meteorological Council this instrument, which was mentioned in the last Report, was, in March, placed in the hands of Captain Abney, R.E., F.R.S., that gentleman having kindly offered to make some experiments with it.

Exposure of Thermometers.—Experiments have been for some time in progress at the Observatory with the view of determining the relative merits of different patterns of thermometer screens. For this purpose there have been erected on the lawn a Stevenson's screen, of the ordinary English pattern, and a large wooden cage, containing a Wild's screen, of the pattern employed in Russia. Each of these screens contains a dry and wet bulb thermometer, and a maximum and minimum, all of which are read daily at 9 A.M. and 9 P.M., their indications being compared with those of the thermograph at the same hours. A third portable metal screen, designed by Mr. De La Rue for use on shipboard, which contains a dry bulb thermometer only, is also carried out by the observer, and read at the same time as the fixed instruments.

The cost of these experiments is borne by the Meteorological Council.

Glycerine Barometer.—Mr. J. Jordan, having obtained a grant from the Government Fund of the Royal Society for the construction of a glycerine barometer, applied to the Committee for permission to erect it in the Observatory. This they willingly granted, and they also undertook to read it regularly for one year. It has accordingly been set up in the building, but owing to its great length, thirty feet, some difficulty was experienced in finding a suitable situation for it. Eventually it was decided to fix the cistern (which was first ascertained to be constructed of non-magnetic materials), in the magneto-

graph room, to lead the tube of composition metal up through the entrance hall, and to put up the upper part of glass, with the verniers and divided scales, in the north library in a convenient position for reading.

It was successfully filled, by Mr. Jordan, with glycerine (coloured red), and has since its erection been read five times daily, simultaneously with the standard mercurial barometer.

Bogen's Barometers.—Mr. F. Bogen has deposited two of the patent standard cistern siphon barometers, described by him in the "Quarterly Journal Met. Soc.," vol. v, p. 137, in the Observatory, with the request that comparisons might be made between the disk reading and the ordinary method of reading barometers. Mr. Bogen visited the Observatory, on several occasions, to give instructions as to the proper method of filling, setting up, and reading his instruments.

Mr. Bogen has also submitted for trial an improved Artificial Horizon.

The De La Rue Evaporation Gauge.—The Vice-Chairman of the Committee has devised a small evaporation gauge, by means of which the water given off from a continually-wetted sheet of vegetable parchment is measured daily. Two of these instruments, constructed by Messrs. Negretti and Zambra, are now at Kew, and their indications have been noted every day, at 10 A.M., for the past six weeks.

The De La Rue Anemograph.—The Meteorological Council, having now in use Thomson's Harmonic Analyser, instructed the Superintendent to again test the anemograph designed by Mr. De La Rue, for the purpose of indicating at once, without replotting, the horizontal movement of the wind from hour to hour.

The instrument, when tried at the Observatory in 1872, was found to be somewhat defective in its working, as, owing to the mechanical arrangements for returning the pencil to zero, its indications were left unrecorded for intervals varying from three to four minutes every hour. An electrical attachment has now been substituted for part of the mechanism, so as to reduce the time lost to about twenty seconds hourly, and the instrument is at present working in the experimental house.

The spare Barograph, belonging to the Meteorological Office, is also erected in the experimental house, for the purpose of trying various photographic processes suggested from time to time as desirable substitutes for those now employed in the preparation of the curves for the registering instruments.

At the request of Professor Stokes, a series of comparisons has been made between a Hodgkinson's Actinometer, returned to England for repair by Mr. Hennessey of Calcutta, and the three standard instruments retained in this country for reference.

Pendulum Observations.—The Committee have had their attention directed to an ambiguity in a paper by Messrs. B. Stewart and Loewy,

on the corrections to be applied to Pendulum Observations printed in the "Proc. Roy. Soc.," vol. xvii, p. 488.

It is there stated, on p. 489, that the observations were made with a *reversible* pendulum, and on p. 497, with Kater's *invariable* pendulum.

After a careful investigation the Committee find that only one pendulum was employed throughout the whole series of experiments, and that pendulum was Kater's *convertible* pendulum, designated in the Royal Society Catalogue of Scientific Apparatus as "the basis of the present system of British Weights and Measures."

The original observations stated in the paper to be preserved at the Kew Observatory cannot at present be found, and the authors are unable to afford the Committee any information as to what has become of them.

Verifications.—The following magnetic instruments have been verified, and their constants have been determined:—

A Dip-circle, by Barrow and Co., for the Austro-Hungarian Government.

A Dip-circle, by J. Dover, for Dr. Guido Schenzl, Budapest.

A pair of Dipping-needles for Professor Smirnow, Kasan.

There have also been purchased on commission and verified:—

A Unifilar, Dip-circle, and Fox Circle, together with the apparatus for the determination of temperature and induction co-efficients of Magnets, for Professor Greene, Bureau of Navigation, Washington.

A Unifilar for Professor Houzeau, Brussels Observatory.

A Unifilar and Dip-circle for Professor Young, Princetown, United States.

A Unifilar for Senhor Capello, Lisbon Observatory.

A Bifilar for Captain Wille, Norwegian Navy.

A Dip-circle for Lieutenant van Hasselt, Utrecht.

A pair of Dipping-needles for Professor Ferrari, Rome.

And a Tabulator for Don Cecilio Pujazon, San Fernando.

There are at present undergoing verification a Dip-circle for Dr. Mielberg of Tiflis, and a Dip-circle for Mr. Casella.

Although there has been a slight falling off in the total number of barometers verified as compared with last year, yet as regards thermometers the result is highly satisfactory, the number of these instruments having considerably increased. This is due to the great demand for verified clinical thermometers.

The branch still continues under the charge of Mr. T. W. Baker, who is assisted by Messrs. J. Foster, T. Gunter, and C. Taylor. The following are the numbers of instruments examined:—

Barometers, Standard	68
„ Marine and Station	77
Aneroids.....	51
Total.....	196
Thermometers, ordinary Meteorological	1286
„ Standard	67
„ Mountain	43
„ Clinical	3405
„ Solar radiation	27
Total.....	4828

Besides these 53 Deep-sea Thermometers have been tested, the greater number of which were subjected in the hydraulic press, without injury, to strains exceeding three and a-half tons on the square inch.

8 Thermometers have been compared at the freezing point of mercury.

Duplicate copies of corrections have been supplied in 54 cases.

12 Standard Thermometers have been calibrated and divided, a set of three being specially constructed for the experiments on Air Thermometers now in progress under Dr. Thorpe at Leeds.

2 Thermograph Tubes were examined for the Zi-Ka-Wei Observatory, Shanghai.

A pair of special Thermometers have been selected, verified, and forwarded to the Royal Observatory, Naples.

The following miscellaneous instruments have also been verified:—

Hydrometers.....	120
Anemometers.....	2
Rain Gauges.....	6
Sextants	4
Theodolites.....	5

There are at present in the Observatory undergoing verification, 26 Barometers, 400 Thermometers, and 4 Rain Gauges.

The comparison of Messrs. Hottinger's Aneroids being completed, the Superintendent drew up an account of the instruments and their performance, which he read before the Meteorological Society.

The paper was published in the "Quarterly Journal," vol. v, p. 189, and the Aneroids were transferred to the care of Mr. Hicks, whom Messrs. Hottinger have appointed their agent in this country.

Several attempts made to verify Thermometers at high temperatures having failed, it is intended to construct a special apparatus

when its pattern shall have been definitely decided upon. Experiments for this purpose are still in progress.

The Committee finding that the "Hall Mark" for Thermometers was not adopted by opticians, owing to the fact that a charge was made for engraving it, decided, in December, to mark all Thermometers verified at the Observatory free of cost.

This resolution having been advertised in the leading scientific journals, and a special assistant, H. Clements, having been engaged for the purpose, the K. O. monogram and register number have been etched upon every Thermometer tested since the beginning of the present year.

As the "Hall Mark" advertisements caused numerous inquiries to be made respecting the verification of instruments, the Committee took advantage of the opportunity to issue a new and revised scale of fees chargeable for this service.

Air Thermometer.—The construction of the Standard Air Thermometer has been delayed, in order that the Committee might be enabled to adopt certain improvements which have been suggested by Professors Thorpe and Rücker, who are at present engaged in making an elaborate and accurate comparison between the mercurial and air thermometers.

At the invitation of Professor Thorpe, Mr. Whipple recently visited the Yorkshire College of Science, Leeds, in order to witness the experiments in progress.

The difference between the old Royal Society Standard Barometer and the Kew standards having been well determined, the daily comparisons were brought to a close in March last. The former instrument is to be shortly returned to Burlington House, and again set up in the apartments of the Royal Society.

Owing to an accident to the Galton Thermometer-testing apparatus, and the increased risk incurred in marking the Thermometers, the breakage of Thermometers has been slightly larger than usual during the past year, but it has only amounted to about one-half per cent. of the total number verified.

Some very old Standard Thermometers having been recently tested, the Superintendent has been instructed to draw up an account of the changes in their indications, for presentation to the Royal Society.

A new form of thermometer has been designed by Mr. Whipple, and constructed by Mr. Hicks, for the purposes of conveying indication of temperature to a distance by means of electricity without the necessity of employing intervening clockwork. Professor W. G. Adams has kindly consented to make a set of experiments with this apparatus in the laboratory of King's College.

Waxed Paper, &c., supplied.—Waxed paper has been supplied to the following Observatories :—

Bombay, Mauritius, Montsouris, San Fernando, and four Observatories of the Meteorological Council.

Anemograph sheets have also been sent to the Mauritius Observatory, and blank forms for the entry of magnetic observations to the Horton Observatory, the Bureau of Navigation, Washington, and the Owens College.

Loan Exhibition.—With the exception of the Hodgkinson's Actinometer and the three instruments mentioned in last year's report, the instruments specified in the Report for 1876 still remain in charge of the Science and Art Department, South Kensington.

Anemometer Testing.—The Committee have had before them the question of the desirability of erecting a suitable apparatus for the testing of Anemometers and Air-meters; but in the opinion of Dr. Robinson it will be better to postpone its erection for a time.

Mauritius Observatory.—The Earl of Carnarvon, Secretary of State for the Colonies, having requested the Committee to nominate a suitable person to the appointment of First Assistant at the Royal Alfred Observatory, Mauritius, they eventually selected Mr. Herbert Irving Bell, Laureate of the Royal College of Mauritius in 1875, from amongst a number of candidates.

The Crown Agents having approved of their choice, Mr. Bell went through a course of training in observatory work, both at the Kew Observatory and at the Royal Observatory, Greenwich, before he sailed for Mauritius on October 6th.

Workshop.—The several pieces of Mechanical Apparatus, such as the Whitworth Lathe and Planing Machine, procured by Grants from either the Government Grant Funds or the Donation Fund for the use of the Kew Observatory, have been kept in thorough order, and many of them are in constant, and others in occasional, use at the Observatory, but the funds of the Committee do not at present allow of the employment of a mechanical assistant, although one is much needed.

Library.—During the year the Library has received, as presents, the publications of

11 English Scientific Societies and Institutions,
46 Foreign and Colonial Scientific Societies and Institutions.

A large number of scientific works have also been presented to the Library by General Sir E. Sabine, and duplicate copies of works already in the Observatory have been presented to

Dr. Hinrichs.
Professor Karlinski.
The Observatory, Pekin.
Mr. Rundell.
The Society of Telegraph Engineers.

The Committee have also received back from the Council of the British Association, a number of works on Meteorology originally presented to the Observatory, but transferred to the Library of the British Association at Albemarle Street in 1871, when the control of the Observatory was placed in the hands of the Royal Society.

It is in contemplation to make a complete catalogue of all the scientific works in the Library, which now number about 8,000.

Ventilation Experiments.—The tubes, cowls, and appliances used by the Sanitary Institute of Great Britain in their experiments on the ventilating power of cowls of different form, still remain fitted up in the experimental house, but no use has been made of them for some months. As, however, there is no immediate necessity for their removal, the Institute has not yet been called upon to dismount them.

Observatory and Grounds.—The buildings and grounds have been kept in repair, but owing to the great outlay caused in replacing the windows and skylights, which were broken in the hailstorm of August 3rd, 1879, Her Majesty's Commissioners of Woods and Forests have not repainted the Observatory this year, although it is greatly needed both internally and externally.

The Workshops have been painted externally, and partially whitened inside.

Application was made to the Commissioners to repair the path across the Park, which had become impassable in the long-continued wet weather; as however they did not accede to the request of the Committee, it has been found necessary to adopt temporary measures for facilitating the approach to the Observatory.

Staff.—The Staff employed at Kew is as follows:—Mr. G. M. Whipple, B.Sc., Superintendent; T. W. Baker, First Assistant; J. Foster, J. W. Hawkesworth, H. McLaughlin, F. G. Figg, E. G. Constable, T. Gunter, J. Hillier, C. Taylor, A. Dawson, H. Clements, and J. Dawson.

Mr. R. W. F. Harrison resigned his appointment in December, and two Junior Assistants, C. Taylor and A. Dawson, were then engaged in his stead.

Visitors.—The Observatory has been honoured by the presence, amongst others, of:—

The President, Officers, and Members of the Council of the Meteorological Society.
Dr. Abbe.
Captain I. du T. Bogle, R.E.
Mr. Gill.
M. Hagemann.
Captain Maxwell, R.N.

Abstract. Kew Observatory Receipts and Payments Account from November 1, 1878, to October 31, 1879.

Dr. **RECEIPTS.**

	£	s.	d.
To Balance from 1877–78	620	12	7
Royal Society (Gassiot Trust) £246 16 7			
,, ,, 248 17 5	495	14	0
Meteorological Office 110 8 4			
,, ,, 100 0 0			
,, ,, 100 0 0			
,, ,, 100 0 0	410	8	4
Meteorological Office, for Postages, &c.	14	15	3
Payment for Instruments on Commission	451	14	7
Sale of Waxed Paper	75	9	2
Verification Fees, Magnetic Instruments 29 18 6			
,, ,, Meteorological Office 64 9 0			
,, ,, Opticians, &c. 311 0 1			
Sale of Standard Thermometers 7 12 0			
Payments for Copying Registers 56 17 6	469	17	1
Mr. De la Rue for Sun-work	104	1	3
Sanitary Institution of Great Britain	10	4	1
Professor Balfour Stewart	4	6	3
	£2657	2	7

Cr. **PAYMENTS.**

	£	s.	d.	£	s.	d.
By Salaries	£983	9	0			
Extra Payments	110	12	4	1094	1	4
Rent of Land	11	11	0			
Fuel and Gas	66	1	9			
Furniture and Fittings	16	2	9			
Chandlery, &c.	10	3	5			
Painting and Repairs	27	13	6			
Maintenance of Path and Fence	16	6	8	147	19	1
Printing and Stationery	45	17	8			
Postages	14	14	7			
Library	14	3	8			
Messenger and Housekeeper	61	14	0			
Advertisements	16	14	0			
Porterage and Contingencies	16	19	5	170	3	4
Instruments purchased on Commission				458	13	10
Postages and Payments on behalf of Meteorological Office				29	4	3
Purchase of Waxed Paper, Packing ditto, &c.				63	2	0
,, ,, Chemicals	28	15	10			
,, ,, Tubes for Standard Thermometers	3	18	0			
Verification Department Expenses (Ice, Carbonic-Acid Gas).	12	2	7			
Repair of Instruments, and Purchase of New	17	17	2			
Carpenter's Work and Sundries	4	3	9	66	17	4
Sun-work expenses				102	0	0
Construction of Instruments for Experiments				4	11	0
Payments in behalf of Sanitary Institute of Great Britain				2	13	1
,, ,, Professor Balfour Stewart				4	6	3
,, ,, Mr. Jordan				1	5	6
Balance—Bank of England	437	16	3			
London and County Bank	60	0	0			
Cash in hand	14	9	4	512	5	7
				£2657	2	7

November 13, 1879. Examined and compared with the Vouchers, and found correct. (Signed) WARREN DE LA RUE, *Auditor.*

ASSETS.

	£	s.	d.
By Balance as per Statement	512	5	7
Meteorological Office, Allowances and Sundries	54	3	0
Commissions	158	3	6
Waxed Paper sold	2	5	0
,, ,, in stock	21	0	0
Blank Forms	3	3	0
Verification Fees due	76	15	0
Standard Thermometers sold	5	0	0
,, ,, in stock	90	11	6
Sanitary Institute of Great Britain	2	13	1
Mr. Jordan	1	5	6
	£927	0	3

LIABILITIES.

	£	s.	d.
To Gas, Fuel, and House Account	15	1	1
Apparatus and Chemicals	5	9	2
Commissions	156	18	6
Balance	749	16	5
	£927	0	3

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N., Long.* 0^h 1^m 15^s·1 *W., for the year October* 1878 *to September* 1879.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnet N E being employed for the purpose.

The Dip observations were made with Dip-circle No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log $\pi^2 K$ employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations,* corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is −0·00179.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected about a quarter of a mile north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have all been made and reduced by Mr. F. G. Figg.

* A vibration is a movement of the magnet from a position of maximum displacement on one side of the meridian to a corresponding position on the other side.

Observations of Deflection for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$. Mean.	Observer.
1878.	d. h. m.	foot.				
October.....	28 12 24 P.M.	1·0	50·3	15 37 12		F.
		1·3	....	7 2 38	9·13057	"
	2 28 „	1·0	52·2	15 36 1		"
		1·3	....	7 2 10		"
November.....	25 12 28 P.M.	1·0	54·9	15 35 14		F.
		1·3	....	7 1 47	9·13014	"
	2 24 „	1·0	52·9	15 35 18		"
		1·3	....	7 1 48		"
December.....	23 12 37 P.M.	1·0	28·2	15 37 54		F.
		1·3	....	7 2 54	9·12967	"
	2 36 „	1·0	31·0	15 37 50		"
		1·3	....	7 2 48		"
1879.						
January.....	28 12 17 P.M.	1·0	34·6	15 39 6		F.
		1·3	....	7 3 29	9·13046	"
	2 19 „	1·0	34·2	15 38 36		"
		1·3	....	7 3 16		"
February	25 12 27 P.M.	1·0	40·3	15 37 41		F.
		1·3	....	7 2 49	9·13043	"
	2 27 „	1·0	40·4	15 38 5		"
		1·3	....	7 3 10		"
March	26 12 25 P.M.	1·0	37·0	15 37 51		F.
		1·3	....	7 3 0	9·13001	"
	2 22 „	1·0	37·1	15 37 2		"
		1·3	....	7 2 33		"
April.....	28 12 17 P.M.	1·0	52·7	15 35 22		F.
		1·3	....	7 1 42	9·12988	"
	2 26 „	1·0	56·2	15 34 7		"
		1·3	....	7 1 12		"
May	26 12 22 P.M.	1·0	63·9	15 33 20		F.
		1·3	....	7 0 53	9·12994	"
	2 28 „	1·0	61·9	15 33 25		"
		1·3	....	7 1 9		"
June	25 12 30 P.M.	1·0	61·4	15 33 15		F.
		1·3	....	7 0 52	9·12962	"
	2 33 „	1·0	65·6	15 32 7		"
		1·3	....	7 0 24		"
July	28 12 24 P.M.	1·0	72·3	15 31 32		F.
		1·3	....	7 0 7	9·12961	"
	2 13 „	1·0	72·7	15 31 8		"
		1·3	....	6 59 50		"
August	28 12 28 P.M.	1·0	65·4	15 32 49		F.
		1·3	....	7 0 37	9·12974	"
	2 27 „	1·0	66·8	15 32 5		"
		1·3	....	7 0 37		"
September.....	26 12 29 P.M.	1·0	62·7	15 33 11		F.
		1·3	....	7 1 0	9·12974	"
	2 36 „	1·0	64·9	15 32 38		"
		1·3	....	7 0 27		"

Vibration Observations for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Temperature.	Time of one Vibration.	Log mX. Mean.	Value of m.*	Observer.
1878.	d. h. m.	°	secs.			
October.....	28 11 38 A.M.	48·9	4·6328			F.
	3 2 P.M.	53·4	4·6317	0·31198	0·52634	„
November.....	25 11 43 A.M.	53·4	4·6367			F.
	3 0 P.M.	51·3	4·6366	0·31128	0·52567	„
December.....	23 12 5 P.M.	26·1	4·6258			F.
	3 8 P.M.	32·3	4·6293	0·31162	0·52558	„
1879.						
January.....	28 11 34 A.M.	33·3	4·6335			F.
	2 53 P.M.	34·0	4·6317	0·31093	0·52564	„
February	25 11 48 A.M.	38·3	4·6317			F.
	3 1 P.M.	39·4	4·6334	0·31118	0·52578	„
March.....	26 11 37 A.M.	35·1	4·6328			F.
	2 58 P.M.	37·3	4·6328	0·31096	0·52539	„
April.....	28 11 33 A.M.	50·2	4·6382			F.
	3 8 P.M.	56·1	4·6372	0·31107	0·52539	„
May.....	26 11 37 A.M.	62·1	4·6419			F.
	3 2 P.M.	61·2	4·6401	0·31094	0·52534	„
June	25 11 55 A.M.	62·8	4·6403			F.
	3 35 P.M.	68·5	4·6415	0·31137	0·52540	„
July.....	28 11 32 A.M.	72·3	4·6418			F.
	2 55 P.M.	73·9	4·6408	0·31181	0·52567	„
August	28 11 40 A.M.	64·5	4·6452			F.
	3 4 P.M.	67·6	4·6456	0·31061	0·52501	„
September.....	26 11 44 A.M.	61·6	4·6406			F.
	3 13 P.M.	68·1	4·6410	0·31138	0·52548	„

* m = magnetic moment of vibrating magnet.

Dip Observations.

Month.	G. M. T.	Needle.	Dip. North.	Observer.	Month.	G. M. T.	Needle.	Dip. North.	Observer.
1878.	d. h. m.	No.	° ′		1879.	d. h. m.	No.	° ′	
Oct.	29 2 59 P.M.	1	67 43·09	F.	Apl.	29 3 16 P.M.	1	67 42·09	F.
	2 59 „	2	42·88	„		3 16 „	2	42·81	„
	30 3 3 „	1	42·72	„		30 2 59 „	1	42·87	„
	3 2 „	2	42·59	„		2 59 „	2	42·37	„
	Mean..	....	67 42·82			Mean..	....	67 42·53	
Nov.	27 3 9 P.M.	1	67 42·50	F.	May	28 3 5 P.M.	1	67 42·25	F.
	3 7 „	2	42·03	„		3 5 „	2	42·56	„
	28 3 1 „	1	42·94	„		29 3 0 „	1	42·03	„
	3 1 „	2	43·24	„		3 1 „	2	42·06	„
	Mean..	....	67 42·68			Mean..	....	67 42·22	
Dec.	30 2 55 P.M.	1	67 42·62	F.	June	27 3 11 P.M.	1	67 43·12	F.
	2 58 „	2	41·94	„		3 9 „	2	42·75	„
	31 3 3 „	1	43·56	„		30 3 6 „	1	41·88	„
	3 2 „	2	43·31	„		3 6 „	2	41·18	„
	Mean..	....	67 42·86			Mean..	....	67 42·23	
1879.									
Jan.	29 3 1 P.M.	1	67 42·93	F.	July	29 3 6 P.M.	1	67 42·62	F.
	3 2 „	2	42·68	„		3 8 „	2	41·68	„
	31 2 53 „	1	43·00	„		30 3 3 „	1	42·43	„
	2 52 „	2	42·87	„		3 5 „	2	42·18	„
	Mean..	....	67 42·87			Mean..	....	67 42·23	
Feb.	26 3 2 P.M.	1	67 43·06	F.	Aug.	28 3 51 P.M.	1	67 42·75	F.
	3 2 „	2	42·37	„		3 51 „	2	42·25	„
	27 3 8 „	1	42·43	„		29 3 16 „	1	41·68	„
	3 8 „	2	41·19	„		3 14 „	2	41·50	„
	Mean..	...	67 42·26			Mean..	....	67 42·04	
Mar.	28 4 27 P.M.	1	67 42·99	F.	Sept.	29 2 55 P.M.	1	67 42·87	F.
	4 25 „	2	42·75	„		2 55 „	2	42·75	„
	31 3 7 „	1	42·87	„		30 3 10 „	1	42·87	„
	3 7 „	2	41·62	„		3 10 „	2	42·93	„
	Mean..	....	67 42·56			Mean..	....	67 42·85	

Month.	Declination.	Magnetic Intensity.								
		English Units.			Metric Units.			C. G. S. Measure.		
	Mean of Observations.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West.									
1878.	° ′ ″									
October	19 10 3	3·8967	9·5076	10·2752	1·7967	4·3838	4·7377	0·1797	0·4384	0·4738
November	19 8 57	3·8956	9·5036	10·2712	1·7962	4·3820	4·7359	0·1796	0·4382	0·4736
December	19 9 37	3·8992	9·5139	10·2821	1·7978	4·3867	4·7409	0·1798	0·4387	0·4741
1879.										
January	19 8 29	3·8925	9·4977	10·2646	1·7948	4·3793	4·7328	0·1795	0·4379	0 4733
February.....	19 8 9	3·8938	9·4960	10·2634	1·7954	4·3784	4·7323	0·1795	0·4378	0·4732
March.....	19 11 14	3·8947	9·5006	10·2679	1·7958	4·3806	4·7343	0·1796	0·4381	0·4734
April	19 6 51	3·8958	9·5032	10·2707	1·7963	4·3818	4·7357	0·1796	0·4382	0·4736
May.....	19 8 57	3·8949	9 4986	10·2662	1·7959	4·3797	4·7336	0·1796	0·4380	0·4734
June	19 7 54	3·8983	9·5067	10·2750	1·7974	4·3834	4·7376	0·1797	0·4383	0·4738
July.....	19 5 7	3·9003	9·5117	10·2804	1·7984	4·3857	4·7401	0·1798	0·4386	0·4740
August	19 10 21	3·8943	9·4955	10·2631	1·7956	4·3782	4·7322	0·1796	0·4378	0·4732
September	19 5 41	3·8978	9·5104	10·2783	1·7972	4·3851	4·7391	0·1797	0·4385	0·4739

APPENDIX II.

Meteorological Observations.—Table I.

Kew Observatory.

Longitude $0^h\ 1^m\ 15^s{\cdot}1$ W. Latitude 51° 28′ 6″ N. Height above sea-level =34 feet.

Mean Monthly results from the continuous Records for the Twelve Months ending September 30th, 1879.

Months.	Thermometer.*						Barometer.†					Pressure.	
	Means.	Extreme maximum.		Extreme minimum.		Means.	Extreme maximum.		Extreme minimum.		Means.		
		Date.	Ther.	Date.	Ther.		Date.	Bar.	Date.	Bar.	Vapour-tension.	Dry air.	
	°	d. h.	°	d. h.	°	inches.	d. h.	inches.	d. h.	inches.	inch.	inches.	
1878. October....	51·1	5 2 P.M.	70·8	31 6 A.M.	31·3	29·786	13 10 A.M.	30·314	26 9 A.M. 10 „	28·989	·315	29·471	
November..	40·0	25 11 A.M.	52·4	29 midt.	29·0	29·755	19 9 P.M.	30·447	15 5 P.M.	29·167	·209	29·546	
December..	33·9	30 noon	53·6	24 10 P.M.	14·7	29·727	24 8 „	30·345	19 6 A.M.	29·121	·175	29·552	
1879. January....	32·6	1 2 A.M.	51·3	12 7 A.M.	21·0	30·034	27 8 „	30·417	3 9 „	29·310	·153	29·881	
February...	38·7	9 1 P.M.	52·5	24 ?	20·8	29·547	1 1 „	30·142	10 6 P.M.	28·797	·208	29·339	
March.....	41·3	19 2 „	62·9	2 7 „	25·8	29·991	8 7 A.M.	30·592	27 4 P.M.	29·527	·209	29·782	
April.....	43·8	26 2 „	60·0	12 1 „	28·9	29·696	29 1 P.M.	30·299	7 7 A.M.	28·897	·228	29·468	
May	48·7	21 6 „	66·0	10 4 „	31·2	30·011	4 8 A.M.	30·489	28 1 „	29·505	·259	29·752	
June.....	57·1	14 1 „	69·7	{4 4 „ 5 4 „}	43·0	29·812	13 9 P.M. 10 „ 10 A.M.	30·128	3 6 „	29·369	·379	29·433	
July.....	58·4	29 2 „	77·0	11 3 „	48·0	29·805	28 11 „ 1 P.M.	30·141	20 8 P.M.	29·265	·399	29·406	
August	60·1	15 4 „	74·7	31 midt.	45·3	29·843	31 0 midt.	30·295	27 6 „	29·428	·425	29·418	
September..	55·8	3 4 „	68·1	26 6 A.M.	40·3	29·973	1 9 P.M.	30·459	24 5 A.M.	29·259	·375	29·598	
	46·8	....	..	....	..	29·832	....	..	....	..	·278	29·554	

The above Table is extracted from the Quarterly Weather Report of the Meteorological Office, by permission of the Meteorological Council.

* The thermometer-bulbs are 10 feet above the ground.

† Readings reduced to sea-level.

† Readings reduced to sea-level.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *.			Weather †. Number of days on which were registered						Wind ‡. Number of days on which it blew.							
		Total.	Maximum.	Date.	Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1878.		in.	in.															
October ..	6·5	2·110	0·415	9	16	1	1	..	4	11	1	..	6	2	7	6	6	3
November	7·9	2·515	0·400	27	16	..	1	..	1	17	11	5	1	..	1	2	4	6
December	7·8	1·315	0·200	31	16	6	..	..	3	20	7	2	3	1	2	9	3	4
1879.																		
January ..	7·6	2·705	1·030	1	10	4	..	..	3	19	1	15	4	2	1	3	4	1
February .	8·6	4·125	0·890	20	22	8	..	..	1	21	4	2	5	1	2	9	3	2
March ...	7·1	0·950	0·355	30	13	3	..	1	6	17	4	6	3	1	3	8	5	1
April	8·1	3·045	0·510	9	18	2	1	1	..	20	7	4	3	2	6	3	3	2
May	7·2	3·945	1·785	28	19	..	2	1	2	18	9	2	3	1	3	8	2	3
June	7·5	4·180	0·610	24	23	..	..	2	..	18	..	..	2	1	10	12	3	2
July	8·4	4·105	1·260	19	21	..	..	1	..	17	..	1	2	1	3	13	8	3
August ..	7·6	4·880	0·850	19	20	..	1	3	1	17	1	1	3	1	6	15	2	2
September	7·0	2·865	1·135	23	16	..	1	1	2	15	4	3	5	1	2	9	5	1
Totals..		36·740			210	24	7	10	23	210	49	41	40	14	46	97	48	30

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † Derived from observations made at 10 A.M., noon, 2, 4, and 10 P.M.
‡ As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Sunshine.*		Maximum temperature in sun's rays.†			Horizontal movement of the Air.‡		
	Total number of hours.	Number of hours Sun was above the horizon.	Mean.	Highest.	Date.	Average daily Velocity.	Greatest Movement in a day.	Date.
1878.	h. m.	h. m.	deg.	deg.		miles.	miles.	
October	112 54	328 53	98·1	121·8	8	208	510	10
November	52 6	264 29	71·2	91·1	8	254	604	10
December	19 48	243 3	57·7	83·3	18	201	632	31
1879.								
January	22 30	258 50	51·9	80·0	15	291	737	8
February	43 12	277 18	76·6	106·4	21	266	463	9
March	94 18	366 19	90·6	119·2	29	278	585	23
April	81 0	414 2	106·2	120·4	26	218	473	11
May	110 48	481 45	115·2	129·5	12	227	402	15
June	121 30	494 18	125·6	134·6	9	254	428	21
July	98 36	497 18	120·6	141·0	29	255	490	8
August	153 54	450 1	119·1	137·1	14	250	456	27
September	117 30	378 10	108·4	123·5	10	177	402	9

* Registered by the Sunshine-recorder.

† Derived from the means of the indications of four black-bulb thermometers in vacuo, read daily at 10 A.M., excepting for the last two months, when a single thermometer only was read.

‡ As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

Presents, November 20, 1879.

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Presents, November 27, 1879.

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December 11, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor Joseph David Everett was admitted into the Society.

The President announced that he had appointed as Vice-Presidents:—

The Treasurer.
Mr. Busk.
Sir Joseph Hooker.
Mr. Simon.
Dr. Tyndall.

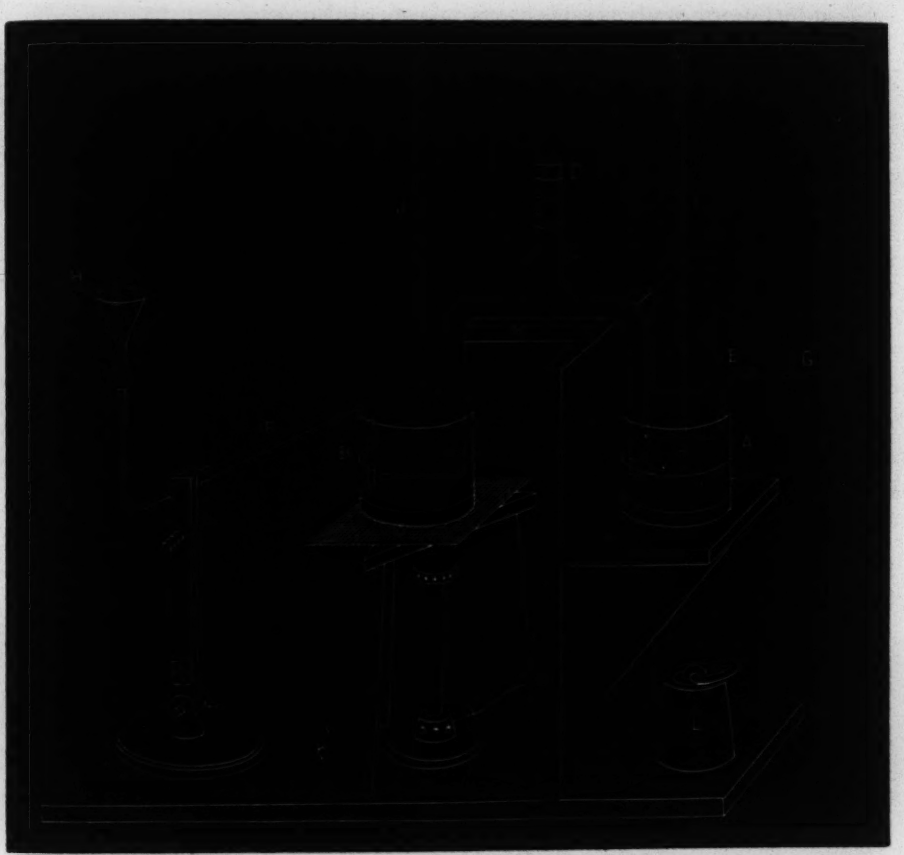
The following Papers were read:—

I. "Thermo-Electric Behaviour of Aqueous Solutions with Mercurial Electrodes." By G. GORE, LL.D., F.R.S. Received November 24, 1879.

In order to investigate this subject, I devised and constructed the following apparatus:—A and B are two thin glass basins, 81 millims. internal diameter (=5,153 sq. millims. of mercury surface), and 6·0 centims. deep; each containing a layer of mercury about 1·0 centim. deep, covered by a layer, about 3 centims. deep, of the aqueous solution to be examined. C is a thin bent glass tube, about 3 centims. diameter, filled with the liquid, and closed by means of a bung at D.* E and F are stout glass tubes, about 2 millims. bore, E having a platinum wire sealed into it at its lower end, and F being filled with mercury; the ends of these tubes are usually placed in the mercury previous to adding the liquid; and F is fixed so that the solution cannot enter it. G and H are terminal wires of platinum, touching the mercury, but not the liquid. I and J are thermometers, with their bulbs immersed in the pools of mercury. Each cup is closed by a bung (not shown in the sketch), to prevent loss of liquid by evaporation, also to support the thermometer and keep the tubes steady. The dish B is supported on wire gauze, placed upon an iron tripod with a Bunsen's burner beneath, and A is placed on a shelf level with

* It is not necessary to have the vertical limbs of this tube of as great a length as they are shown in the sketch.

it. K K K K is a wooden frame for supporting the apparatus, and L is a weight to keep it steady. This kind of apparatus was used in all the experiments, and acted very satisfactorily. I devised one of different construction for a similar purpose many years ago. (*See* "Phil. Mag." June, 1859.)



Liquid Thermo-Pair with Mercury Electrodes.

The mercury employed was prepared by distilling the ordinary metal twice, then shaking it strongly in succession with dilute nitric acid, water, solution of cyanide of potassium, distilled water, and filtering it. Distilled water was used in making all the solutions.

In using the apparatus, the mercury was first poured into the basins; the tubes E and F placed, taking particular care that the mercurial column in F and its contact with the pool were complete. Two very shallow india-rubber bungs were next inserted in the lower ends of the bent tube C, the tube then put in position, and filled with the liquid. The solution was now put into the basins; the bung D inserted tightly, the shallow ones then removed, and the bungs inserted in A and B. The completeness of the circuit was next tested by attaching a galvanometer to the instrument, and, if necessary, a

minute swing of the needle was obtained by repeated connexion at proper intervals of time. B was now heated. About seven or eight minutes was usually occupied in raising its temperature the required number of degrees; the deflections at the various temperatures being noted. After an experiment had been made, the mercury and solution of both vessels were mixed and cooled, and the mercury filtered, before repeating the trial with the same liquid.

Care was taken to select solutions which had the least amount of chemical action upon the mercury; those of salts of the heavy and less positive metals were therefore not employed. No liquid was used which produced any visible film or dulness upon that metal when heated with it. It was found necessary to heat each solution to about 180° F., and keep it at that temperature some little time to expel dissolved air, and then cool it previous to making experiments, otherwise the results obtained in repetition experiments were not quite uniform. In a previous research on the thermo-electric properties of liquids ("Proc. Roy. Soc.," No. 188, 1878, pp. 513—543), in which platinum electrodes were employed, such previous heating was less necessary, probably because the thermo-electric currents were then stronger.* After heating and cooling the liquid, it was freely shaken with the mercury, and then filtered, previous to making experiments with it. With every liquid, one or more repetition experiments were made in order to obtain uniform results, and confirm their reliability; the amount of difference of deflection of the needles in such experiments did not exceed one degree. The galvanometer employed was an astatic one, having two coils, each of 50 ohms resistance, the two being connected and used as one of 100 ohms resistance.

I have ascertained by separate experiments of a different kind, that mercury when sufficiently agitated with solutions, neutral to test paper, of salts of the alkali metals, renders some of those liquids feebly alkaline; the effect, however, is so slight, requires such extensive and long-continued contact of the substances, that it appears consistent with the view that chemical action is not the cause of the currents in these thermo-electric experiments.

The following are the results obtained with different liquids in the above apparatus.

Behaviour of different Solutions.

Experiment No. 1. Ten ounces of water, and $2\frac{1}{2}$ grs. of potassic cyanide of a high degree of purity. Cold mercury was positive 2°

* The greater strength of the currents with the platinum electrodes was partly due to the circumstance that each platinum electrode offered a surface of 71.680 sq. millims. of contact with the liquid, or fourteen times greater than that of the above-mentioned mercurial surface. The solutions employed were also much stronger.

steadily at 180° F.* Agitation either of the cold mercury (A), or of the hot mercury (B), did not alter the amount of the deflection.

No. 2. A solution† of 5 grs. of the cyanide. Cold mercury positive 14°. Agitation either of A or B had no effect.

No. 3. A solution of 50 grs. of the same salt. Cold mercury positive 45°. Stirring A decreased the deflection temporarily‡ to 34°, and stirring B decreased it to 9.

No. 4. A solution of 100 grs. of the same salt. Cold mercury positive 50°. Stirring A decreased the deflection to 41, and stirring B decreased it to 46.

No. 5. A solution of 10 minims of selenic acid (of sp. gr. 1·32 at 60° F.). Hot mercury positive $1\frac{1}{2}$°. Stirring A increased the deflection to 5, and stirring B increased it to 4. There was a similarity of behaviour of this liquid to that of dilute sulphuric acid, each exhibited a zero point giving no current at about 120° F.; but the selenic acid gave a reverse current below that temperature, whilst sulphuric acid did not.

No. 6. A solution of 20 minims of the same acid. Cold mercury positive 3° at 120° F. Needles at 0 at 143° F., and hot mercury positive $3\frac{3}{4}$° at 180° F. Stirring A decreased the deflection to $1\frac{1}{4}$, and stirring B increased it to $5\frac{3}{4}$.

No. 7. A solution of 25 grs. of anhydrous selenate of sodium. Hot mercury positive 8°. Stirring A decreased the deflection to 5, and stirring B increased it to 14.

No. 8. A solution of 100 grs. of crystalline sulphate of ammonium. Hot mercury positive $12\frac{1}{4}$°. Stirring A increased the deflection to $29\frac{1}{4}$, and stirring B increased it to $19\frac{1}{4}$.

No. 9. A solution of 50 grs. of crystalline ammonic alum. Cold mercury positive 2°. Stirring A increased the deflection to $5\frac{1}{2}$, and stirring B decreased it slightly.

No. 10. A solution of 100 grs. of the same salt. Cold mercury positive 4°. Stirring A had no effect; stirring B reversed the deflection 2°.

No. 11. A solution of 40 minims of formic acid of sp. gr. 1·18. Hot mercury positive $3\frac{1}{2}$°. Stirring A had no effect; stirring B increased the deflection to 7.

No. 12. A solution of 80 minims of the same acid. Hot mercury positive 5°. Stirring A increased the deflection to 6, and stirring B increased it to 7.

No. 13. A solution of 100 grs. of potassic nitrate. Hot mercury

* All the deflections, except those otherwise stated, were taken at the temperature of 180° F.

† Unless otherwise described, 10 ozs. of water was used in every case to form the solution.

‡ All the alterations of deflection caused by stirring were only temporary.

positive 10°. Stirring A increased the deflection to 30, and stirring B increased it to 20.

No. 14. A solution of 100 grs. of crystalline potassic sulphate. Hot mercury positive 12°. Stirring A increased the deflection to 30, and stirring B increased it to 22.

No. 15. Seventy-nine volumes of water, and one of pure sulphuric acid. Cold mercury positive $1\frac{1}{4}°$. Stirring A increased the deflection to 10, and stirring B increased it to 4.

No. 16. Thirty-nine volumes of water, and one of the same acid. Hot mercury positive $6\frac{3}{4}°$. Stirring A increased the deflection to 11, and stirring B increased it to 13.

No. 17. Nineteen volumes of water, and one of the same acid. Cold mercury positive, $14\frac{1}{2}°$ at 120°, and $6\frac{1}{4}°$ at 180° F. Stirring A increased the deflection to 30, and stirring B increased it to 19.

No. 18. A solution of 50 grs. of pure and partly dehydrated* sodic carbonate. Hot mercury positive 19°. Stirring A increased the deflection to 41, and stirring B increased it to 25.

No. 19. A solution of 50 grs. of pure and partly dehydrated potassic carbonate. Hot mercury positive 12°. Stirring A increased the deflection to 18, and stirring B did not affect it.

No. 20. A solution of 100 grs. of crystalline sodic sulphate. Hot mercury positive $2\frac{1}{2}°$. Stirring A increased the deflection to $8\frac{1}{2}$, and stirring B increased it to 5.

No. 21. A solution of 100 grs. of ammonic nitrate. Hot mercury positive $4\frac{1}{4}°$. Stirring A increased the deflection to 16, and stirring B increased it to 20.

No. 22. A solution of 100 grs. of crystalline boracic acid. Hot mercury positive 2°. Stirring either A or B did not alter the amount of deflection.

No. 23. A solution of 100 grs. of sodic nitrate. Hot mercury positive 6°. Stirring A increased the deflection to 30, and stirring B increased it to 18.

No. 24. A solution of 100 grs. of crystalline phosphate of sodium. Hot mercury positive 8°. Stirring A increased the deflection to 28, and stirring B increased it to 21.

No. 25. A solution of 50 grs. of baric nitrate. Hot mercury positive $7\frac{1}{2}°$. Stirring A increased the deflection to 22, and stirring B increased it to 16.

No. 26. A solution of 50 grs. of crystalline nitrate of strontium. Hot mercury positive 10°. Stirring A increased the deflection to 14, and stirring B increased it to 17.

No. 27. A solution of 100 grs. of crystalline phosphate of ammonia. Hot mercury positive 28°. Stirring A increased the deflection to 38, and stirring B increased it to 32.

* See page 482.

No. 28. A solution of 50 grs. of crystals of borax. Hot mercury positive $10\frac{1}{2}°$. Stirring A increased the deflection to 22, and stirring B increased it to 15.

No. 29. A solution of 50 grs. of crystalline sodic hyposulphite. Cold mercury positive 4°. Stirring A decreased the deflection to 3, and stirring B had no effect.

No. 30. A solution of 10 grs. of crystals of sodic sulphite. Hot mercury positive $2\frac{1}{2}°$. Stirring A increased the deflection to 5, and stirring B had no effect.

No. 31. A solution of 50 grs. of the same salt. Hot mercury positive 13°. Stirring A increased the deflection to 15, and stirring B had no effect.

No. 32. A solution of 50 grs. of potassic sulphite. Cold mercury positive 9°. Stirring A increased the deflection to 13, and stirring B made hot mercury positive 12°.

No. 33. A solution of 50 grs. of crystalline phosphate of sodium and ammonium. Hot mercury positive 5°. Stirring A increased the deflection to 9, and stirring B increased it to 7.

No. 34. A solution of 100 grs. of the same salt. Hot mercury positive 2°. Stirring A increased the deflection to 4, and stirring B had no effect.

No. 35. A solution of 50 grs. of formiate of sodium. Hot mercury positive 4°. Stirring A increased the deflection to 20, and stirring B increased it to 10.

No. 36. A solution of 100 grs. of the same salt. Hot mercury positive 6°. Stirring A increased the deflection to 10, and stirring B increased it to 11.

By arranging the foregoing results in a series with the solution at the top, in which the hot mercury was the most positive at 180° F., and that at the bottom in which it was most negative, the following order was obtained. The horizontal line separates those liquids in which the hot metal was positive from those in which it was negative.

	Amount of deflection.
1. Phosphate of ammonium	28·0+
2. Carbonate of sodium	19·0
3. Sulphite of sodium (50 grs.)	13·0
4. Sulphate of ammonium	12·75
5. Carbonate of potassium	12·0
6. Sulphate of potassium	12·0
7. Borax	10·5
8. Nitrate of potassium	10·0
9. Nitrate of strontium	10·0
10. Selenate of sodium	8·0
11. Phosphate of sodium	8·0

	Amount of deflection.
12. Nitrate of barium	7·5
13. Nitrate of sodium }	6·0
14. Formiate of sodium (100 grs.) }	6·0
15. Microcosmic salt (50 grs.) }	5·0
16. Formic acid (80 minims) }	5·0
17. Nitrate of ammonium	4·25
18. Formiate of sodium (50 grs.)	4·0
19. Selenic acid (20 minims)	3·75+
20. Formic acid (40 minims)	3·5
21. Sulphate of sodium }	2·5
22. Sulphite of sodium (10 grs.) }	2·5
23. Microcosmic salt (100 grs.) }	2·0
24. Boracic acid }	2·0
25. Selenic acid (10 minims.)	1·5
26. Sulphuric acid (1 in 80)	1·25−
27. Ammonia alum (50 grs.) }	2·0
28. Cyanide of potassium ($2\frac{1}{2}$ grs.) }	2·0
29. Hyposulphite of sodium }	4·0
30. Ammonia alum (100 grs.) }	4·0
31. Sulphuric acid (1 in 20)	6·25
32. Sulphuric acid (1 in 40)	6·75
33. Sulphite of potassium	9·0
34. Cyanide of potassium (5 grs.)	14·0
35. Cyanide of potassium (50 grs.)	45·0
36. Cyanide of potassium (100 grs.)	50·0

To ascertain whether this order agreed with that of a series arranged according to the different degrees of electromotive force of the various couples, I simultaneously employed two apparatuses similar to that already described (but with basins of 75 millims. internal diameter), charging one of them with one solution, and the other with another which was next to it in the above series, connecting them with the two wires respectively of the differential galvanometer, so that their currents circulated in opposite directions through the coil of that instrument, and ascertaining which gave the strongest current at 160° F. By this means the following series was obtained, in which the couple giving the strongest positive current is at the top, and that giving the most powerful negative current at the bottom, as in the previous list. The horizontal line separates those liquids in which the hot metal was positive from those in which it was negative. The relative positions of the solutions in the two series, although they agree in the main, are not identical; for instance, nitrate of potassium occupies the eighth place in the first series, and the second place in

the second series; phosphate of ammonium, sulphate of ammonium, &c., are also changed in position. Such changes were probably affected by the circumstance, that whilst in the first series the electrodes were 81 millims. diameter (=5,153 sq. millims. of surface), and the current from a single apparatus circulated through a continuous double length of wire in the coil of the galvanometer, in the second series the electrodes were only 75 millims. diameter (=4,418 sq. millims.), and the current from a single apparatus circulated through a single half length of the coil. The temperatures at which the deflections were noted were also different in the two series. The surface of one electrode in the smaller apparatus was about $\frac{1}{16}$ and in the larger one $\frac{1}{14}$ of that of one of the platinum electrodes in the apparatus already referred to (see foot note, p. 474).

	Difference of amount of deflection.
1. Sulphate of ammonium	1·0+
2. Nitrate of potassium	3·0
3. Phosphate of ammonium	3·0
4. Sulphite of sodium (50 grs.)	0·75
5. Carbonate of sodium	1·0
6. Carbonate of potassium.....	1·5
7. Sulphate of potassium	0·25
8. Nitrate of strontium	0·25
9. Phosphate of sodium	0·25
10. Borax	1·5
11. Selenate of sodium.....	1·5
12. Nitrate of barium	0·25
13. Nitrate of sodium	1·0
14. Formiate of sodium (100 grs.).....	1·0
15. Microcosmic salt (50 grs.).....	0·75
16. Formiate of sodium (50 grs.)	0·75
17. Formic acid (80 minims)	2·0
18. Nitrate of ammonium	1·5
19. Sulphate of sodium	0·25
20. Sulphite of sodium (10 grs.)	0·25
21. Microcosmic salt (100 grs.)	2·0
22. Boracic acid.....	
23. Ammonia alum (50 grs.)	very slight—
24. Cyanide of potassium ($2\frac{1}{2}$ grs.)	0·75
25. Ammonia alum (100 grs.)	1·0
26. Hyposulphite of sodium.....	1·0
27. Cyanide of potassium (5 grs.)	13·0
28. Cyanide of potassium (50 grs.).....	2·25
29. Cyanide of potassium (100 grs.)	

Remarks.—Many of the remarks which I have made in a previous paper on the thermo-electric properties of liquids (Proc. Roy. Soc., vol. xxvii, pp. 535–538) respecting the cause of the currents, apply to the results obtained in the present research; and I consider that the causes of the currents and of their direction are similar in the two cases.

On examining the results it will be observed that the effects are not manifestly related to the chemical nature of the solutions; thus, the liquids in which hot mercury is positive include those of acid, as well as those of alkaline reaction; and similarly with the solutions in which cold mercury is positive. To what extent chemical action occurred in the experiments and operated as a cause of electric currents, would be a difficult point to completely prove, because the amount required to produce such feeble currents would be so extremely small; the result of a general examination of the phenomena, however, does not support the hypothesis that the currents were of chemical origin.

The evident cause of the currents was heat, and the direction of them was probably due to molecular changes in the liquid attendant upon alteration of temperature. In nearly all cases the magnitude of the current increased with the amount of difference of temperature; the apparent, but not real, exceptions were those in which a reversal, or an advance towards it, occurred.

The strength of the solution manifestly affected the quantity, and in some cases the direction of the current, as may be perceived with the solutions of potassic cyanide (Nos. 1, 2, 3, and 4), selenic acid (Nos. 5 and 6), ammonia alum (Nos. 9 and 10), formic acid (Nos. 11 and 12), sulphuric acid (Nos. 15, 16, and 17), and phosphate of sodium and ammonium (Nos. 33 and 34). If the cause of the current, in the case of sulphuric acid, was chemical action, the direction of the current would probably not have been reversed by mere alteration of temperature, or by variation of strength of the mixture. If, also, the cause of the currents in general was of a chemical nature, the currents would probably have been much stronger, especially with the stronger solutions, and would have increased in all cases, with the strength of the solution, until the latter was much more concentrated than those which were employed.

Stirring either portion of the liquid influenced the deflection in 59 cases out of 72, increased it in 49, and decreased it in 10 of those cases. Stirring the cold portion altered the current in 31 cases out of 36, and stirring the hot portion changed it in 28 cases out of 36. Stirring the cold portion also had a greater average magnitude of effect upon the current than stirring the hot portion. Stirring a liquid of acid reaction increased the current in 13 cases out of 20, decreased or reversed it in 3 cases, and had no effect in 4 cases. Stirring an alkaline one increased the deflection in 12 cases out of 24, decreased it

in 5, and had no effect in 7. The effects of stirring upon the amount of deflection were greater in alkaline liquids than in those of acid reaction.

Influence of previously heating the Solution.

Having observed, even with solutions formed with water which had been preboiled to expel dissolved air, that unless such liquids had been further heated to 180° F. (and cooled), after the salt, &c., had dissolved, the currents obtained in successive experiments with such a solution were not quite uniform in amount, but always decreased, I examined this circumstance.

Twenty ounces of distilled water was boiled and cooled; the salt to be employed was then dissolved completely in it, and the solution shaken with pure mercury. The mixture was then divided into two equal portions, one of those portions raised to a temperature of 180° F. in a covered glass vessel, and the evaporated water condensed and returned to the mixture, so that the solution remained unaltered in composition; the liquid was then cooled. These preheated and non-preheated portions were employed respectively to charge the two apparatuses already described. The two thermo-pairs were then simultaneously excited, their currents allowed to circulate in opposite directions through the coils of the galvanometer, and the deflection noted when the hot portions of the two liquids had simultaneously acquired a temperature of 160° F. The following is a brief statement of the results.

Twenty ounces of distilled water, and 200 grs. of crystals of sodic phosphate. The non-preheated portion of solution yielded the strongest current; deflection 1°.

Twenty ounces of water and 2 drms. of sulphuric acid. The non-preheated portion gave the strongest current; deflection ·25°.

Twenty ounces of water and 100 grs. of partly dehydrated sodic carbonate. The non-preheated portion gave the strongest current; deflection ·33° at 140° F., and ·25° at 160° F.

Twenty ounces of water and 200 grs. of baric nitrate. The non-preheated portion yielded the strongest current; deflection 1°.

The same mixture as the last; one portion of the solution was preheated with mercury, instead of being preheated alone. The non-preheated portion gave the strongest current; deflection 1° at 180° F.

In each of these five experiments the non-preheated portion of liquid yielded the current of greatest electromotive force. This accords with the original observation that repetition experiments always yielded weaker currents (see p. 480).

In order to ascertain whether preheating the one portion of salt before dissolving it had the same effect upon the current as heating it after having been dissolved, the following experiments were made:—

Preboiled and cooled 20 ozs. of distilled water, and divided into two exactly equal portions. Took two portions of 50 grs. each of partly dehydrated sodic carbonate; heated one portion to 180° F. for fifteen minutes until it ceased to evolve moisture (it lost 12·4 grs.), and cooled it. Dissolved the two separate portions of salt in the separate quantities of water, and tested the solutions against each other in the two apparatuses. No difference of strength of current occurred at any temperature up to that of 180° F.

In another experiment of this kind, 200 grs. of ammonia alum were divided and treated as above. The solution of the non-preheated portion gave the strongest current; deflection ·75° at 180° F.

Remarks.—The conclusion I draw from these last two experiments and results, combined with those of the previous ones, is that preheating the salt either before or after solution usually weakens the current, both in thermo-electropositive and in thermo-electronegative liquids.

In the several classes of cases described in this paper, it is evident that the currents are not due to chemical action, but to heat acting upon and altering particular molecular structures, and that the difference of electromotive power in different liquids is not due to differences of chemical action, but probably to differences of molecular arrangement of the solutions. Also that in the class of experiments where the liquids compared had the same chemical composition, but had been differently treated, the differences of molecular arrangement were less, and the currents obtained were consequently more feeble. A difference of strength of current caused by repetition of experiments, or by continuance of heat, also renders manifest the change of molecular arrangement; and the above method may be employed for detecting molecular differences in conducting liquids having the same chemical composition.

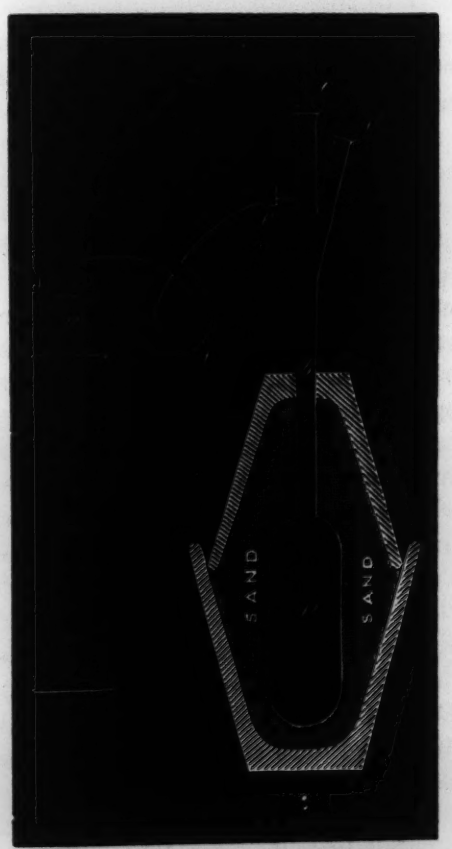
In the class of cases in which the differences of molecular arrangement were the least, and the currents the most feeble, the direction of the currents was the most uniform. This is in accordance with the common truth in science that the smallest phenomena are the most constant.

II. "Quantitative Spectroscopic Experiments." By Professor G. D. Liveing, M.A., F.R.S., and Professor James Dewar, M.A., F.R.S. Received November 27, 1879.

The well-known expansion of the lines of sodium and other metals, when increased quantities of the metals are introduced into the flame or arc, seems to afford a means of attaining a quantitative spectrum analysis in certain cases, if the relations between the width of

the lines and the thickness, density, and temperature of the vapours could be determined. We had such a determination in view during our experiments on the reversal of the lines of metallic vapours heated in iron tubes, and have since made a good many experiments expressly for the purpose. We have not yet overcome all the difficulties attending these experiments; nevertheless, some results, definite and interesting, have been reached, which we now communicate to the Society.

The first experiments were made with wrought iron bottles, of the form shown in the accompanying figure. To prevent the diffusion of carbonic oxide from the fire we found it essential to have the wide



part, *a*, and part of the tube, *b*, coated with borax. The bottle was then packed in sand in two crucibles, one of them inverted, and heated in a small furnace fed with gas retort carbon. The bottle was previously treated with fused caustic potash at a red heat, and washed with acid. Any oxide remaining in the bottle was reduced by a current of hydrogen introduced by the side tube *e*, and forced to pass to the bottom by allowing it no exit except by a narrow tube passed through the opening *c* and reaching nearly to the bottom of the bottle, as

shown by the dotted lines. The hydrogen was then replaced by nitrogen, the narrow tube removed, *c* closed by a glass plate through which the spectrum was viewed, and the side tube *e* connected by a thick rubber tube with a glass tube for collecting the gas driven out by the metallic vapour, or with a manometer formed of an inverted burette dipping into a vessel of water. The sodium, weighed in dry carbonic acid gas, was introduced in little iron cups about the size of gun caps through the oblique side tube *d*, and the orifice instantly closed with a rubber stopper, for the purpose of measuring the gas expelled as the sodium evaporated. This orifice was fitted with a screw cap, for use when the pressure of the gas in the bottle was to be varied.

Attempts to measure the temperature of our furnace were not very successful. Observations of temperature depending on the electromotive force of a thermo-electric junction of platinum and palladium, which had been carefully calibrated in boiling mercury, boiling cadmium, and boiling zinc, failed at the temperature at which our experiments were made from the fusion of the palladium. This indicates, from Violle's experiments, that the temperature of the furnace was about 1,500° C.

The bottles, however carefully cleaned, were never quite free from sodium, so that on observing, at first, the spectrum emitted by the hot bottom of the bottle, the D absorption was seen as two fine lines.

On introducing a fragment of sodium (·015 or ·020 grm.) the following phenomena were observed with the spectroscope. First D appeared as a narrow *bright* band, considerably brighter than the general spectrum of the bottle, with the fine dark pair of lines quite unexpanded in its midst; this bright band rapidly expanded, becoming fainter and more diffuse as it expanded, was visible to a width equal to fourteen or sixteen times the distance of the D lines from each other, and then died away entirely, leaving the fine dark lines much as they were at first. The addition of several pieces of sodium in succession produced a repetition of the same phenomena, the D absorption lines being but very little wider after the introduction of sodium enough to fill the bottle with its vapour.

Larger pieces of sodium produced an increased width of expansion of the bright band; but from the very diffuse character of the edges of the band and from its being seen against a background only a little less bright, it was not possible to get any exact measures of the widths to which the bands expanded.

On observing the gas expelled from the bottle, it was found that the expansion of the bright band was synchronous with the expulsion of gas, and ceased when gas ceased to come out; that is to say, it was synchronous with the expansion of the sodium vapour within the bottle. It was found, however, that when the gas ceased to be ex-

pelled from the bottle, it almost immediately began to be sucked back, and that nearly, if not quite, as rapidly as it had been expelled, until not only the whole gas expelled was drawn in again, but generally a small quantity of nitrogen in addition. In a few seconds, not only had the sodium vapour ceased to have any sensible tension, but a portion of the nitrogen (2 to 3 cub. centims.) had been absorbed. There was no absorption of nitrogen before the sodium was put in, and when the equilibrium had been attained, which was always in less than a minute, there was no absorption afterwards.

These phenomena seemed to indicate, (1), either that the temperature of the sodium vapour was higher than that of the iron, or that the emissive power of sodium vapour for light of the wave-length of D was greater than that of iron at the temperature of the furnace; (2), that the width of the bright D line, or band, increased with the depth of the stratum of sodium when the density was not increased, inasmuch as the pressure to which the sodium vapour was subject was always the same, and the only, or the principal, change during the expansion of the sodium vapour in the bottle was in the thickness of the layer of vapour; (3), that iron either has the power of forming a compound with sodium, which is very stable at the temperature of the furnace, or else it has a power of transmitting sodium vapour analogous to that which platinum has in regard to hydrogen. This last supposition seems to be negatived by the fact that we found that out of ·220 grm. of sodium put into the bottle in a series of experiments on one occasion, ·198 grm. was afterwards extracted when the bottle was cold by washing it out with hydrochloric acid. The effect was not due to the nitrogen, since the sodium appeared to be taken up in the same way when carbonic oxide was substituted for nitrogen.

This behaviour of iron towards sodium explains a phenomenon which had puzzled us when operating before with iron tubes, viz., that it required a very large quantity of sodium, many times the normal amount, to fill one of the tubes with sodium vapour. It also seems to explain the fact observed by Mr. Lockyer (Proc. Roy. Soc., vol. xxii, p. 371), that sodium heated in a long iron tube gives the D absorption line "no thicker than when seen under similar conditions in a test tube."

We came, of course, to the conclusion that it was useless to pursue the investigation of the quantitative measure of sodium vapour by experiments in iron vessels; and we had a stout platinum tube made, half an inch in diameter and 23 inches long, connected by a union joint with an iron tube with fittings similar to those of the top of the iron bottles before described; and also a bottle similar to the iron bottles, but entirely of platinum. When used, the tube was packed with magnesia inside a wider porcelain tube, and the bottle was packed with lime in blacklead crucibles.

For convenience in introducing the sodium, a fine platinum tube, about 1 millim. in diameter, was filled with sodium by sucking up the melted metal, and different lengths of this tube were cut off as required, wrapped at once in thin platinum foil, and dropped into the heated vessel. Amalgams containing various percentages of sodium were also employed. In the case of amalgams containing 1 per cent. and upwards the pieces were weighed; those containing less than 1 per cent. were measured by a very fine pipette. In all cases the amalgam was introduced into the hot vessel wrapped in platinum foil.

For varying the pressure within the vessels, and so by compression or expansion varying the density and thickness of the stratum of metallic vapour under observation, an ordinary compressing and exhausting syringe, connected with a gauge, was used. With the bottle we could not venture to reduce the pressure below that of the atmosphere, lest the softened platinum should collapse; but in the case of the tube the pressure could be reduced to one-fourth of an atmosphere. Into both vessels we have pumped nitrogen until the pressure reached three atmospheres.

Whenever it was desired to empty the vessel of sodium, in order to begin a fresh series of experiments without the necessity of cooling the vessel, the sodium (or sodium and mercury) vapours were blown out by a current of hydrogen passed down to the bottom of the vessel in the mode already described, and when the metallic vapours were sufficiently expelled, the hydrogen was replaced by nitrogen. In this way, though the last traces of sodium were never expelled, the D absorption could be reduced to two narrow lines.

On introducing a fragment of sodium into the hot vessel, when free, or nearly so, from sodium to begin with, the *bright* yellow band rapidly expanding and becoming fainter and more diffuse, and finally dying away, was seen as in the iron bottles; but instead of the D absorption remaining narrow as it had been before, it began to increase in width as the bright band died away, and soon attained a maximum width, and remained steadily at that width—a width several times that of the distance between the D lines, but much less than that of the bright band at its widest. A second piece of sodium thrown in produced a repetition of the same phenomena, except that the bright band was obscured by the absorption of the layer of sodium vapour first introduced, and only seen when it expanded beyond the absorption band. More pieces of sodium produced like effects; the absorption band gradually widening as more sodium was put in, until the channelled spectrum began to appear.

The effects of compressing the vapour were very remarkable. As the pressure increased the channelled spectrum speedily disappeared, then the diffuse edges of the D band contracted, the band itself likewise contracting until it became a very fine pair of lines,

or if the amount of sodium present was not too much, D came out bright. On letting off the pressure, the phenomena recurred in the reverse order, and the whole could be repeated several times. After compression as long as the pressure was sustained the D absorption remained permanently narrowed, but did not continue bright. When there was only a very little sodium in the platinum tube and the pressure diminished, the D band was seen to expand and become diffuse, contracting again as the pressure was restored, but not generally contracting to quite its former width or sharpness.

We used sometimes amalgams of sodium in order to have a very dense gas to retard diffusion, and for the purpose of getting various quantities of sodium diffused through equal spaces. With these amalgams the general course of the phenomena was much the same as with the metal alone, except that we could not by compression to three atmospheres make the D lines show out so bright as with sodium alone, although the absorption was reduced to extremely narrow dimensions.

One set of experiments was made with a series of amalgams containing decreasing percentages, all less than 1 per cent., of sodium, equal quantities of each were put into the hot tube in succession, the vapour of each being blown out before the next was put in. The thickness of the layers observed was thus very nearly constant, but the density of the sodium vapour variable. It was calculated that the mercury vapour would, at the temperature of the tube, fill a height of about 20 centims. The D absorption was very broad, with wide diffuse edges, with an amalgam of $\frac{1}{2}$ per cent., and did not sensibly diminish in width until the percentage of sodium was reduced to $\frac{1}{8}$.

The results of the foregoing experiments may have been complicated by the sodium vapour which diffused into the cool part of the vessel. We have attempted to overcome this complication by passing down into the bottle, when full, or nearly full, of sodium vapour, a platinum tube closed at the top with a glass plate, and filled with nitrogen, and observing the absorption through this tube. The nitrogen in the tube prevents for a short time the entry of the sodium vapour into the tube, and so by passing the tube to different depths, the thickness of the layer of sodium through which the observations were made could be varied. It was found in this way that a layer of sodium vapour, about 4 centims. thick at the atmospheric pressure at the temperature of our furnace, gave the D absorption sharp and very narrow, but as the sodium diffused into the tube the absorption extended until it produced a broad band with diffuse edges. Without a long process of washing, it is impossible to clear the tube of sodium, so that the repetition of observations in this way is a very tedious business. We are looking for some transparent substance which will stand the high

temperature and the action of sodium, wherewith to close the lower end of our tube of inspection. We have tried a plate of colourless sapphire, but it very soon became too opaque for further use. Perhaps fused alumina may do better.

A few experiments made in a similar way with thallium in iron bottles, showed on the whole similar results. No bright green line was seen on dropping in a piece of thallium, but it was not until after the lapse of some seconds that the absorption line was apparent. It was faint at first, gradually deepening, and soon attaining a maximum of intensity, when it was remarkably stationary for a long time. More pieces of thallium introduced successively produced a small but perceptible widening of the absorption band and a diminution of the sharpness of its edges.

The foregoing account gives the general course of the phenomena, and it seems useless at present to describe further details, because the quantitative results which we hoped to give are for the time in abeyance until we can ascertain more correctly the temperature of our furnace and the thickness of the layer of vapour observed. This requires an accurate knowledge of the density of the vapour, which could not be satisfactorily deduced from the experiments made with iron bottles, as they did not accord well and were complicated by the action of iron on sodium above described. The platinum vessels stand but few experiments without repair, which causes considerable delay; and the above-mentioned results have necessitated the expensive operation of the reconstruction of some five platinum bottles.

The phenomena attending the compression of the vapours, as well as those of the amalgams of varying percentages, seem to indicate that the width of the D absorption is dependent on the thickness and temperature of the absorbent vapour rather than on the whole quantity of sodium present in it. Very minute quantities diffused into the cool part of the tube appear to give a broad diffuse absorption, while a layer of denser vapour of small thickness in the hottest part of the vessel gives but a very narrow absorption. This may, however, be due to the variation of temperature.

The phenomena of the bright D band seen on compressing the vapour is due to the elevation of temperature consequent on compression. This elevation, we have calculated, for a *sudden* compression to half the original volume, will amount in the case of sodium vapour, if the ratio of the specific heat at constant pressure to that at constant volume be taken as 1·14, to about one-third of the original temperature, whatever that temperature be; so that, allowing for loss during slower compression, the temperature of the vapour in our bottles will have nearly reached 2,000°. In the case of the amalgams the main portion of the gas being mercury vapour, the effect of compression to a like amount will increase the temperature by nearly two-

thirds, in consequence of the ratio of the specific heat at constant pressure to that at constant volume being now 1·66. It is not so easy to explain why the bright band should appear while the sodium is evaporating, when a depression of temperature should ensue. The depression of temperature will, however, affect the hot bottom of the vessel, and so it and the lowest layer of sodium vapour may be relatively cooler than the layers above. Professor Stokes has suggested another cause which may intensify the effect, depending on the fact that the radiation from the sides of the vessel is partly reflective, partly emissive, while that from the sodium is almost wholly emissive. The bottom of the vessel will be somewhat diminished in brightness by the opening opposite giving no radiation to be reflected by it, while the temperature of the sodium being well sustained by the radiation of the sides, the want of radiation from the opening will not make any sensible change in its emission. Whatever explanation be given of the cause, the fact of the gradual expansion of the bright band as the sodium evaporates bears out the supposition that an increased thickness of sodium vapour, with little or no change of density, produces a wider line.

The very minute quantity of sodium required to produce a wide absorption was shown in the experiments with the amalgams. In one of these ·042 grm. of an amalgam containing only $\frac{1}{8}$ per cent. of sodium was introduced into the platinum tube. That quantity of mercury would at 1,500° C., which was about the temperature of the furnace, fill about 25 cub. centims., occupying a depth of about 20 centims. of the tube. In this volume there was only about ·00005 grm. of sodium, yet the D absorption band was at least twice as wide as the space between the two D lines.

The quantity of sodium requisite to give the channelled spectrum also appears to be very small. Between 4 and 5 mgrms. put into the platinum bottle, whether alone or as an amalgam, usually gave the channelled spectrum; sometimes it was seen with even less than 4 mgrms. Now the wide part of the bottle held some 200 cub. centims., so that the quantity of sodium diffused through this space and up into the tube would give a vapour of very small density indeed. The complete disappearance of the channellings as soon as the vapours were a little compressed, so as to be driven down into the hot part of the bottle, seems to indicate that the channellings were due to comparatively cool vapour in the upper part of the vessel.

The facility with which very high temperatures can be obtained by sudden compression, naturally suggests a repetition of our experiments with the electric arc in lime crucibles, under circumstances in which a pressure of a few atmospheres can be suddenly applied; and this we hope to carry out.

III. "Further Experiments on the Vapour Densities of Potassium and Sodium." By Professor JAMES DEWAR, M.A., F.R.S., and ALEXANDER SCOTT, B.A. Received November 27, 1879.

In our former communication "On the Vapour Densities of Potassium and Sodium," we pointed out the chief obstacles which lay in the way of an exact determination of these constants. Having overcome the chief manipulative difficulties in connexion with the method we described, there still remained the problem for solution as to how far the use of iron bottles in our experiments might affect the results. If the iron retained the metals or allowed their vapours to diffuse with rapidity through it, a considerable error might be produced without its being easily detected. We noted an action of this kind, remarking that "both metals seemed to have a tendency to behave in the same manner as zinc, viz., to form an alloy with the iron, because the vapour evolved after a few minutes was absorbed completely, in spite of all precautions to prevent oxidation." The tendency of iron and the alkali metals to form alloys, we thought, would be diminished, so as not to interfere with the value of the results, "by raising the temperature as much as possible, so as to induce rapid volatilisation," and the fair accordance of the series of experiments formerly given led us to suppose that this difficulty had been successfully obviated. Recent spectroscopic observations, by Professors Liveing and Dewar, on the luminous absorption of the vapours of sodium under different conditions in iron and platinum vessels—which will be presented shortly to the Society—having led to the inference that mere rapidity of volatilisation, of such small weights of the metals as can be conveniently employed for the measurement of the volumes of the vapours, in presence of a relatively enormous mass of iron, would not eliminate the absorptive action, new experiments were made in a platinum apparatus of similar shape and size to the iron one, and in the same manner as formerly described. Magnesia was used instead of sand to pack the platinum vessel in the crucibles employed as a protection against the diffusion of furnace gases, and the volatilisation of the alkali metals took place in nitrogen. Each set of experiments was made at different times, and the results are given in the order of observation, the bottle being freed from the vapours after each experiment by a strong current of hydrogen. The molecular weights are calculated for 22·34 cub. centims. at 0° C. and 760 mm. pressure.

The following tables contain the record of the results obtained:—

Potassium.

I.

·046 grm. gave....	24·1 c.c. at 0°	and 760 mm.	Mol. wt...	42·6
·045 „	23·1 „	„	„ ..	43·5
·042 „	21·2 „	„	„ ..	44·2
·045 „	22·9 „	„	„ ..	43·9
·045 „	22·1 „	„	„ ..	45·5
·030 „	25·1 „	„	„ ..	44·5
			Mean molecular weight..	44·03

II.

·047 grm. gave....	26·8 c.c. at 0°	and 760 mm.	Mol. wt...	39·2
·049 „	26·0 „	„	„ ..	42·1
			Mean molecular weight..	40·65

Sodium.

I.

·032 grm. gave	26·5 c.c. 0°	and 760 mm.	Mol. wt.....	27·0
·033 „	26·7 „	„	„	27·6
·031 „	27·9 „	„	„	24·8
			Mean molecular weight....	26·4

II.

·032 grm. gave	25·1 c.c. 0°	and 760 mm.	Mol. wt.....	28·5
·032 „	26·3 „	„	„	27·2
·031 „	27·9 „	„	„	24·8
·029 „	26·5 „	„	„	24·4
·031 „	26·9 „	„	„	25·7
·030 „	27·3 „	„	„	24·6
·030 „	26·1 „	„	„	25·6
			Mean molecular weight....	25·8

III.

·027	grm.	gave	24·6	c.c. at 0° and 760 mm.	Mol. wt.	....	24·5
·027	,,	....	23·2	,, ,,	,,	....	26·0
·037	,,	....	31·7	,, ,,	,,	....	26·1
·030	,,	....	29·1	,, ,,	,,	....	23·0
					Mean molecular weight.	....	24·9

The new determinations of the molecular weights of potassium and sodium are just about half the former values, and would seem to support the inference that the atom of each of these metals resembles mercury and cadmium in the gaseous state, as regards molecular volume. Such a remarkable result cannot be accepted without a very thorough investigation of the secondary reactions which may be induced at high temperatures. It is certain that in the platinum vessels there is much less absorption of vapour than in the case of iron, and that the action of the furnace gases is all but eliminated. The following suggestions might be made to explain the results. In the first place, the metals may contain a large amount of occluded hydrogen. With reference to this assumption, we have examined the sodium used in our experiments and find, on careful exhaustion and boiling in a Sprengel vacuum, that one volume of sodium only contains one-third of its volume of occluded hydrogen, so that this will not account "for the double volume of the atom of the metals." The alkali metals may form volatile compounds with platinum, although, judging from analogy, such compounds are not likely to exist. If so, the vapour blown out of the bottle ought to contain platinum. The use of platinum tubes to blow out the vapours prevented the test being applied. The metals may be changed into oxides from occluded oxygen in the platinum. If so, no free metal ought to remain in the bottle after this rapid evaporation. This however is not the case; so that, unless some other explanation can be given, it will be necessary to admit some anomaly in the behaviour of potassium and sodium, as regards their vapour densities at high temperatures. In future the variation of the density with the temperature must be determined, but this is very difficult at temperatures about 1,500° C. The chief drawback to the prosecution of the experiments is the terrible waste of platinum vessels, which never stand the combined action of potassium, sodium, and hydrogen at high temperatures, beyond two or three operations.

The following determinations of the molecular weight of potassium iodide, made in the iron apparatus, are of importance in connexion with the previous results.

Potassium Iodide.

·159	grm.	gave....	20·75	at 0°	and 760 mm.	Mol. wt.....	171
·165	,,	....	21·2	,,	,,	,, ...	174
·170	,,	....	21·4	,,	,,	,, ...	177
·180	,,	....	21·6	,,	,,	,, ...	186
·103	,,	....	15·1	,,	,,	,, ...	152
·174	,,	....	24·4	,,	,,	,, ...	159

Mean molecular weight.... 169·8

These numbers are fairly accordant, and seem to indicate that iodide of potassium is normal in its behaviour. The vapour, after each experiment, was blown out with a current of hydrogen. A crystalline deposit was obtained in each case, which was pure iodide of potassium, free from any trace of iron. Taken in connexion with the former experiments, this seems to show that, if free potassium is abnormal, its compounds are not altered. Before any final conclusions can be reached, further experiments must be made in platinum or iridium vessels, and it will be very important to control the results by examining the densities of the iodides of cæsium and rubidium.

IV. "On the Practical Solution of the Most General Problems in Continuous Beams." By JOHN PERRY and W. E. AYRTON. Communicated by FLEEMING JENKIN, F.R.S.S. Lond. and Edin., Professor of Civil Engineering in the University of Edinburgh. Received November 27, 1879.

1. It is not necessary to enter into the question of the advisability of employing continuous girders in bridges with spans of less than 200 feet, but it is generally conceded that the increased economy due to the employment of continuous girders in longer spans more than counterbalances the well-known practical objections to continuity. Hence the practical solution of the general problem—given the conditions at the ends of a continuous girder, the spans, the moment of inertia of all cross sections, and the loading, to find the bending moment and shearing stress in every cross-section, is not unworthy of our attention.

2. Mr. Heppel has published the exact solution for cases in which each span may be supposed divided into two, three, four or five equal parts, in each of which the load and cross-section are supposed to be constant. As is well known, the difficulty of the general problem is due to the necessity of making certain summations in each span. Now, Mr.

Heppel found it convenient for his special case to make the summations twice for the same span, working forwards and backwards from every point of support, but in the following the summations are all effected working in the same direction, only *one* summation being necessary for each span.

3. Suppose OQ to be one span of a continuous beam which has loads and supporting forces beyond O and Q. To find the bending moment at a point P, it is sufficient to know what is the bending moment at O, and the shearing force at a very short distance inside O. If we suppose the bending moment to be called positive when it tends to make the beam convex upwards, and the shearing force at a section positive when it tends to elevate the part of the beam to the right of the section, then M, the bending moment at a point P, is equal to

$$\mathrm{M}_0 - \mathrm{S}_0 \times \mathrm{OP} + m,$$

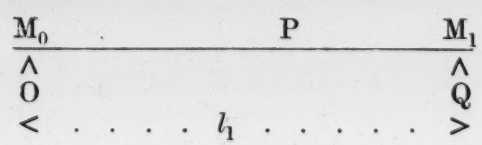
where M_0 and S_0 are the bending moment and shear at O, and where m is the sum of all such expressions as—an element of load at any place P', between O and P, multiplied by $P'P$.

Again, if M is the bending moment at a section, E the modulus of elasticity, I the moment of inertia of the section about its neutral line, y the vertical displacement of a point in the neutral axis of the beam from its horizontal position, x the horizontal distance of this point from some fixed point in the neutral axis as O, then for all beams and girders used by engineers—

$$\frac{d^2y}{dx^2} = \frac{\mathrm{M}}{\mathrm{EI}} \quad \ldots\ldots\ldots \quad (1).$$

The only restriction that we shall use is to suppose all points of support in the same straight line. But as is well known, if this case is solved, the unrestricted case may also be solved, and the case in which there is a yielding in the supports.

Let M_0 M_1 be the bending moments at the points of support O and Q. Let S_0 be the shearing force just inside O. When we know the nature of the loading—



we can find m the moment at P due to the load between O and P. Thus if w be the load per unit length at any place, and if we know how w alters, we can find the sum—

$$\int_0^{\mathrm{OP}} w(\mathrm{OP}-x)dx = \int_0^{\mathrm{OP}} wzdz = m \quad \ldots\ldots \quad (2).$$

And also, if we know how EI alters, we can find the following sums, each of which is denoted by a symbol :—

$$\left.\begin{aligned} \int_0^x \frac{dx}{EI} &= d & \int_0^{l_1} \frac{dx}{EI} &= d_1 \\ \int_0^x \frac{x\,dx}{EI} &= f & \int_0^{l_1} \frac{x\,dx}{EI} &= f_1 \\ \int_0^x \frac{m\,dx}{EI} &= g & \int_0^{l_1} \frac{m\,dx}{EI} &= g_1 \end{aligned}\right\} \quad \ldots\ldots \quad (3),$$

and also

$$\left.\begin{aligned} \int_0^x d\,.\,dx &= n & \int_0^{l_1} d\,.\,dx &= n_1 \\ \int_0^x f\,.\,dx &= q & \int_0^{l_1} f\,.\,dx &= q_1 \\ \int_0^x g\,.\,dx &= \mathrm{F} & \int_0^{l_1} g\,.\,dx &= \mathrm{F}_1 \end{aligned}\right\} \quad \ldots\ldots \quad (4).$$

It is very easy to write out the values of these summations in certain simple cases, and the calculation in the most general cases can always be made with a very fair approximation to accuracy, and with not much risk of mistakes being made, by the graphic methods described below.

The bending moment at any point P is—

$$\mathrm{M} = \mathrm{M}_0 - \mathrm{S}_0 x + m \quad \ldots\ldots\ldots \quad (5),$$

but at Q we have $x = l_1$,

and $\mathrm{M} = \mathrm{M}_1$,

and hence $\mathrm{M}_1 = \mathrm{M}_0 - \mathrm{S}_0 l_1 + m_1$,

therefore $$\mathrm{S}_0 = \frac{\mathrm{M}_0 - \mathrm{M}_1 + m_1}{l_1} \quad \ldots\ldots\ldots \quad (6).$$

So that expression (5) becomes

$$\mathrm{M} = \mathrm{M}_0 + m - x\frac{\mathrm{M}_0 - \mathrm{M}_1 + m_1}{l_1} \quad \ldots\ldots \quad (7).$$

Substituting this value for M in the differential equation (1), and integrating once, remembering that when x equals nought, d, f, and g vanish, we see that if α_0 is the inclination (very small) of the beam downwards from the horizontal at O, and if α is the inclination of the beam at any point P, then

$$\frac{dy}{dx} \text{ or } \tan\alpha \text{ or } \alpha = \alpha_0 + \mathrm{M}_0 d - \mathrm{S}_0 f + g \quad \ldots\ldots \quad (8).$$

If α_1 is the inclination at the support Q, then

$$\alpha_1=\alpha_0+\mathrm{M}_0 d-\mathrm{S}_0 f+g_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

Integrating (8) again we see that the deflection at any point P is

$$y=x\alpha_0+\mathrm{M}_0 n+\mathrm{F}-\mathrm{S}_0 q \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

and, as the supports are on the same level,

$$y=0$$

when $$x=l_1,$$

so that $$0=l_1\alpha_0+\mathrm{M}_0 n_1+\mathrm{F}_1-\mathrm{S}_0 q_1,$$

and hence $$\alpha_0=\frac{\mathrm{S}_0 q_1-\mathrm{F}_1-\mathrm{M}_0 n_1}{l_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

Substituting the value of S_0 from (6) we have therefore

$$\alpha_0=\frac{(\mathrm{M}_0+m_1-\mathrm{M}_1)q_1-l_1\mathrm{F}_1-\mathrm{M}_0 n_1 l_1}{{l_1}^2} \quad . \quad . \quad . \quad . \quad (12),$$

and using this in equation (9) we find

$$\alpha_1=\frac{(\mathrm{M}_0+m_1-\mathrm{M}_1)q_1-l_1\mathrm{F}_1-\mathrm{M}_0 n_1 l_1}{{l_1}^2}+\mathrm{M}_0 d_1-\mathrm{S}_0 f_1+g_1 \quad . \quad (13),$$

Now, let m_2, d_2, f_2, g_2, n_2, q_2, F_2, be the values of the summations made in the next span, QR, Q corresponding with O, and R with Q. If M_2 is the bending moment at the support R, then equation (12) becomes, putting α_1 instead of α_0, M_1 instead of M_0, M_2 instead of M_1,*

$$\alpha_1=\frac{(\mathrm{M}_1+m_2-\mathrm{M}_2)q_2-l_2\mathrm{F}_2-\mathrm{M}_1 n_2 l_2}{{l_2}^2} \quad . \quad . \quad . \quad . \quad (14)$$

M_0		M_1		P		M_2
∧		∧	x			∧
O		Q				R
<	l_1	. . . . . ×		l_2	. . . .	>

Thus we have in equations (13) and (14) distinct values of the angle of inclination at Q. Putting these values equal to one another

* If θ is the small angle of discontinuity of the neutral line of the beam at Q, that is if θ is equal to

$$\left\{\begin{array}{l}\text{inclination downwards of the}\\ \text{neutral line of } QR \text{ at } Q\end{array}\right\}-\left\{\begin{array}{l}\text{inclination downwards of the}\\ \text{neutral line of } OQ \text{ at } Q,\end{array}\right.$$

then we see that α_1 in (14) minus α_1 in (13) equals θ, and hence we must put $-\theta$ instead of 0 on the right hand side of equation (15). The angle θ is not supposed to vary with alteration of loading.

and simplifying, we find an equation connecting M_0, M_1, and M_2, and this is our form of the General Theorem of the Three Moments.*

$$M_0\left(\frac{q_1}{l_1^2}-\frac{n_1}{l_1}+d_1-\frac{f_1}{l_1}\right)-M_1\left(\frac{q_2}{l_2^2}-\frac{n_2}{l_2}+\frac{q_1}{l_1^2}-\frac{f_1}{l_1}\right)+M_2\frac{q_2}{l_2^2}$$

$$+\frac{m_1q_1}{l_1^2}-\frac{m_2q_2}{l_2^2}+g_1-\frac{f_1m_1}{l_1}+\frac{F_2}{l_2}-\frac{F_1}{l_1}=0 \quad . \quad . \quad . \quad . \quad (15).$$

4. We have in the theorem of three moments a relation between the bending moments M_0, M_1, M_2, at any three consecutive points of support; therefore in a continuous girder of N spans there are $N-1$ such equations, and $N-1$ unknown moments, because the moments at the end supports are each equal to nought. Hence the moments at all the supports are found by the easy solution of these simple simultaneous equations.

We may now suppose that the moments at the supports O and Q at the two ends of a span are found, then the shearing force at O can be calculated by equation (6), the bending moment at any point P by equation (7), and the deflection at any point P by equation (10). Points of maximum and minimum bending moment are obtained by equating to nought the differential coefficient of M with respect to x. At a point of inflexion we know that the bending moment must be equal to nought.

We can prove that, in any span OQ of a continuous beam when we know the angles of inclination of the girder at the two points of support, we have sufficient data for making all the necessary calculations. For

$$\alpha_0 \text{ at } O=\frac{(M_0+m_1-M_1)q_1-l_1F_1-M_0n_1l_1}{l_1^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

$$\alpha_1 \text{ at } Q=\frac{(M_0+m_1-M_1)q_1-l_1F_1-M_0n_1l_1}{l_1^2}+M_0d_1-S_0f_1+g_1 \quad (13),$$

so that, knowing a_0 and a_1, we can find M_0 and M_1; and then the bending moment at any point P, if x equals OP, is

$$M=M_0+m-x\frac{M_0-M_1+m_1}{l_1} \quad . \quad . \quad . \quad . \quad . \quad (7),$$

so that we can draw the diagram of bending moment. Also the shear at O is

$$S_0=\frac{M_0-M_1+m_1}{l_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

* Although the theorem of the three moments as given in equation (15) involves more expressions than Mr. Heppel's form as given by Professor Rankine ("Proc. Roy. Soc.," vol. xviii, p. 178), still it will be observed that it is really more convenient. In any case the summations d, f, g have to be calculated in order to obtain n, q, and F, but with our form of the theorem there is this advantage, that when there are more than two spans the summations have only to be made *once* for any span.

and the shear at any point P is equal to S_0 minus the load between O and P, therefore we can draw the diagram of shearing force. Also the deflection at any point P equals

$$M_0 n + F - S_0 q.$$

As a familiar example, we may consider a beam OQ, of which the ends are fixed horizontally and at the same level, here

$$\alpha_0 = 0,$$

and

$$\alpha_1 = 0.$$

We can prove that, when we know the angle of the beam at one end of a span, and the bending moment at the other end, we have sufficient data for calculation, having the above summations m, &c. This applies to the familiar case of a beam fixed horizontally at one end and merely supported at the other end, the supports being level, as before.

The following well-known examples illustrate the use of the formulæ :—

In all cases when the cross-section of the beam remains constant, the summations d, f, n, and q may easily be shown to have the following values :—

$$d = \frac{x}{EI}, \qquad d_1 = \frac{l_1}{EI}, \qquad d_2 = \frac{l_2}{EI},$$

$$f = \frac{x^2}{2EI}, \qquad f_1 = \frac{l_1^2}{2EI}, \qquad f_2 = \frac{l_2^2}{2EI},$$

$$n = \frac{x^2}{2EI}, \qquad n_1 = \frac{l_1^2}{2EI}, \qquad n_2 = \frac{l_2^2}{2EI},$$

$$q = \frac{x^3}{6EI}, \qquad q_1 = \frac{l_1^3}{6EI}, \qquad q_2 = \frac{l_2^3}{6EI},$$

so that the theorem of three moments becomes—

$$M_0 l_1 + 2M_1(l_1 + l_2) + M_2 l_2 - 2m_1 l_1 - m_2 l_2 + 6EI\left(g_1 + \frac{F_2}{l_2} - \frac{F_1}{l_1}\right) = 0.$$

In all cases where the load is spread uniformly over the span, being w per unit length, it is easy to show that—

$$m = \frac{wx^2}{2}, \qquad m_1 = \frac{wl^2}{2},$$

$$g = \frac{wx^3}{6EI}, \qquad g_1 = \frac{wl^3}{6EI},$$

the section being constant on the span.

$$F = \frac{wx^4}{24EI}, \qquad F_1 = \frac{wl^4}{24EI}.$$

When the spans are equal in length, and all cross-sections are the

same, and the loading over both spans is uniform and equal to w per unit length, then the theorem becomes—

$$M_0+4M_1+M_2-\frac{wl^2}{2}=0.$$

When in the two consecutive spans, OQ and QR, there is the same common cross-section, and when there is a uniform load, w, per unit length over the first span, and w_2 over the second, we get the theorem in M. Clapeyron's form—

$$M_0l_1+2M_1(l_1+l_2)+M_2l_2-\frac{w_1l_1^3}{4}-\frac{w_2l_2^3}{4}=0.$$

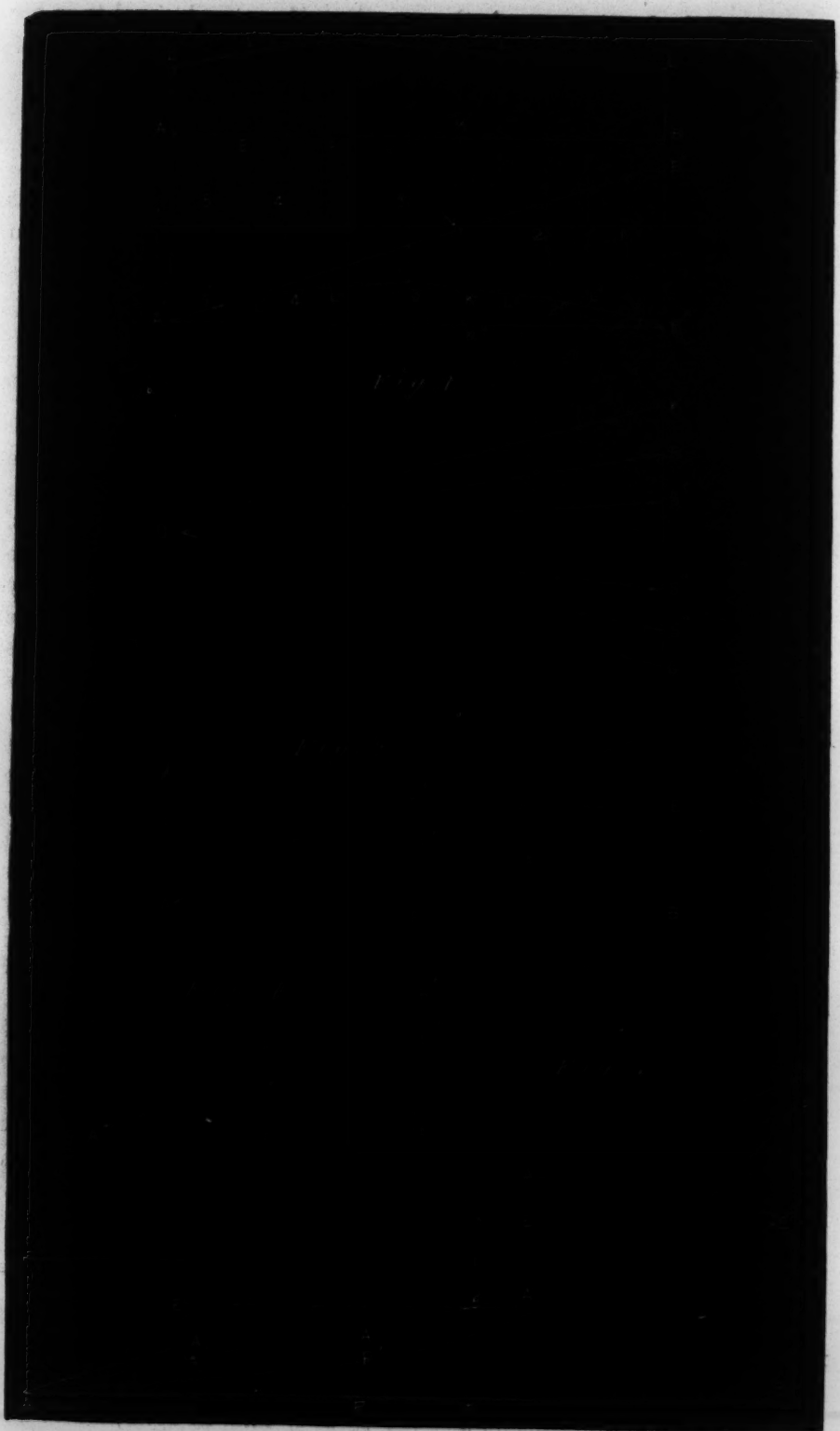
Graphic Determination of the Summations of paragraph 3.

5. It has hitherto been usual for engineers to assume that the load and section are uniform in each span, because, as we have seen, the calculations in this case are easy, whereas those necessary when attempting the general solution are exceedingly complicated; in fact, an examination of Mr. Heppel's solution of a comparatively simple case is sufficient to deter engineers from such calculations. But by the following graphic method the solution of the most general case only requires a very elementary mathematical knowledge, and may be completed in a few hours, or in a much shorter time if a Thomson's integrating machine, or even a good planimeter be available.

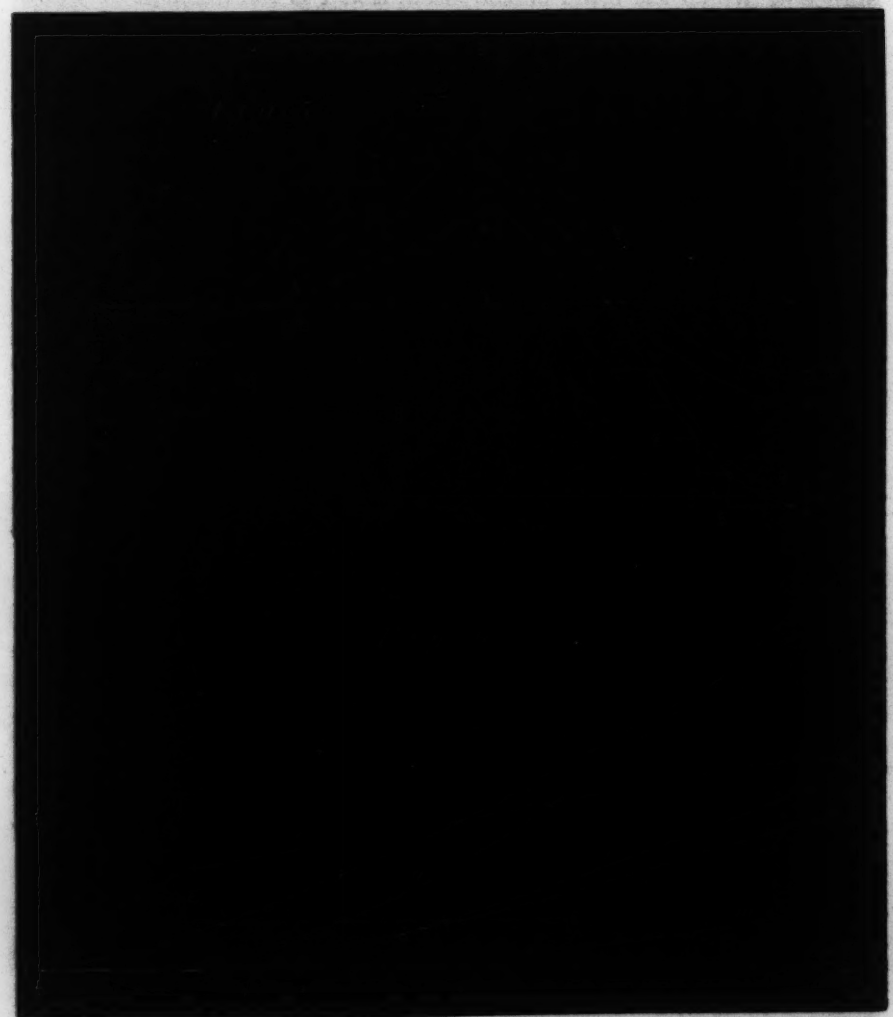
We employ a link polygon method for the calculation of m, and it may be expedient to draw the deflected beam by Mohr's method. To employ, however, a link polygon method throughout the whole of the investigation would lead to much waste of time, however interesting it might be from a mathematical point of view; and as in many other engineering problems, for instance, the finding of centres of gravity and moments of inertia, so in this we see that the use of a little arithmetic, and the actual measurement of lines gives quicker and more accurate integrations.

Our students in Japan made the necessary summations for spans of 300 and 200 feet respectively. Their drawings were exhibited at the meeting, and one of them is shown on a reduced scale in fig. 6. It is advisable to use some well-known scale for the measurement of the lines; for example, a scale of centimetres. The distance AB, fig. 6, represents the span of 200 feet to the scale—one centimetre stands for $3\frac{1}{3}$ feet. The ordinate of the diagram $AEEE$, &c., shows the value of $\frac{1}{EI}$ at every point of the span, and in all cases, except where there is a sudden change of inclination of the beam over a pier (see the end of § 3), any unit of measurement whatever may be employed for $\frac{1}{EI}$.

6. *To find the curve for* m *and the value of* m.—In fig. 1, AB is the



span, and the ordinate of the figure *ALLLB*, shows at any point to a given scale the intensity of the load; that is, the amount of load per foot of the beam at that place. Divide the space *ALLLB* into any convenient number of spaces by vertical lines, and assume that the load on *EF*, for instance, is not a distributed load, but a single one, acting through the centre of gravity of the area *EL'L''F*, and numerically equal to the total load on *EF*, and similarly with the other parts, so



that now the span has concentrated loads 1, 2, 3, 4, 5. Draw *GK* representing the loads to a given scale, and join the points *S* with any point *O*, as shown in the figure. We shall call *GK* our force polygon, as usual, and we now proceed to draw our link polygon, drawing *A'5B''* parallel to *OK*, *5'4'* parallel to *OS*, &c. Then A'5'4'3'2'1'B' is our link polygon for concentrated loads, and for the real loading we must draw a curve through $A'L_1L_2L_3L_4B'$. Producing *A'5'* we shall find

it quite easy to prove that any ordinate, as NK for instance, represents the value of m for the point K', and $B''B'$ is m_1.*

In fig. 6 the diagram for m has been altered in shape by the student, but this extra labour was not necessary.

7. Any ordinate to the curve $AEEEB$, fig. 2, represents the value at that place of $\frac{1}{EI}$, where E is the modulus of elasticity of the substance of which the beam is composed, and I is the moment of inertia of the section about its neutral line, any unit of measurement being employed.

The value of d at any point P is shown by the ordinate PP' to the curve $AP'B'$, and BB' equals d_1, the curve $AP'B'$ being obtained by raising at a number of points, such as P, the ordinate PP', measured to any convenient scale, and representing the area $AEE'P$. It is evident that n_1 is the total area of the figure $AP'B'B$, the scale of measurement being computed in the manner described below.

8. *To find* f.—$A\ E_1\ E_2\ E_3\ B$, fig. 3, shows the value of $\frac{1}{EI}$ at every point. Take $A\ T$ any convenient distance, and raise the perpendicular TT'; make $T3$ equal to BE_3, $T2$ equal to the ordinate at E_2 and $T1$ equal to the ordinate at E_1. Join A with the points, 1, 2, 3, &c., and produce if necessary. Now it is quite evident that any ordinate of the figure $AA_1A_2A_3A_4B''B$ represents $\frac{x}{EI}$ to a known scale; thus, for example, PA' represents PA divided by EI at the point P. (This diagram might, of course, have been easily obtained numerically.) Now at any point make the ordinate PP' represent to any suitable scale the area $AA_1A_2A'P$, and it is evident that any ordinate of the curve through all such points as P', say $AP'B'$, is f to a known scale. It is also evident that g_1 is the total area of the figure $AP'B'B$.†

9. *To find* g.—At any point P we now know (see fig. 1) the value of m; we also know the value of $\frac{1}{EI}$; we can, therefore, compute the

* For we know that the ordinate KK'' represents the bending moment at the point K' if the beam is merely supported at the ends. Also NK'' represents the bending moment about K' due to the proper supporting force at A, and therefore NK, the difference, is m the bending moment at K' due to the loading merely between A and K'.

† Generally as to scale :—When at the point P, fig. 4, we raise a perpendicular PP'' which represents, according to any scale, by its length the area of PAP', then if the ordinate PP' was to such a scale that one centimetre represents a units, and if AB is the span to such a scale that one centimetre represents b feet, if the area AP'P is c square centimetres, and if it is represented by a line PP'' whose length is e centimetres, it is evident that the ordinates of our new curve $AP''B''B$ have a scale such that one centimetre represents $\frac{abc}{e}$ units.

value of $\frac{m}{EI}$ by actual measurement of lines and numerical work; and erect the perpendicular PP', fig. 4, representing $\frac{m}{EI}$ to any suitable scale. Now make $P\,P''$ represent to a suitable scale the area PAP', then the curve passing through all such points as P'' has for its ordinates the values of g. It is also evident that F_1 is the total area of the figure $AP''B''B$.

Example.

10. As an example we may take the following, worked out by Messrs. Terauchi and Saiki, two of our students:—

Design a lattice-girder-bridge with two main girders, the girders being of a constant depth; to carry a double line of railway, and to consist of three spans, respectively 200, 300, and 200 feet long.

Consulting Baker, they assumed the total weight of cross-beams, permanent way, &c., to equal 0·55 ton per foot of the middle span, and 0·5 ton per foot in the end spans: the weight of main girders to be 1·35 and 0·7 ton per foot in the middle and end spans respectively, and the rolling load 2 tons per foot.

First Calculations.—Assume that the girders have everywhere the same cross-section, then we have merely to employ the following equation, § 4, for any two consecutive spans:—

$$M_0 l_1 + 2M_1(l_1 + l_2) + M_2 l_2 - \frac{w_1 l_1^3}{4} - \frac{w_2 l_2^3}{4} = 0.$$

1. *As regards the Weight of the Bridge itself.*

$$w_1 = 1{\cdot}2, \qquad w_2 = 1{\cdot}9, \qquad w_3 = 1{\cdot}2.$$

We find $M_1 = M_2 = 11711{\cdot}5$ ton feet,

$$M_0 = M_3 = 0.$$

At any point in the first span

$$M = 0{\cdot}6x^2 - 61{\cdot}4425x.$$

At any point in the second span

$$M = 11711{\cdot}5 - 0{\cdot}95x^2 - 285x.$$

The curve $OAA'A''A'''$ (fig. 5) has been plotted to show this diagram of bending moments for half the bridge, the other half being exactly similar. O and Q are two supports, and H is the middle of the bridge.

2. *The Rolling Load covering the whole Bridge.*

We find that $M_1 = M_2 = 25173$ ton feet,

$$M_0 = M_3 = 0.$$

At any point in the first span

$$M=1{\cdot}6x^2-194{\cdot}135x.$$

At any point in the second span

$$M=25173+1{\cdot}95x^2-585x,$$

and the diagram for half the bridge is $OBB'B''B'''$,

3. *The Rolling Load covering only the Middle Span.*

We find that $M_1=M_2=22096$ ton feet.

$$M_0=M_3=0.$$

At any point of the first span

$$M=0{\cdot}6x^2-7{\cdot}52x.$$

At any point of the second span

$$M=22096+1{\cdot}95x^2-585x,$$

and the diagram for half the bridge is $OCC'C''C'''$.

4. *The Rolling Load covering the ends Spans only.*

We find that $M_1=M_2=14788$ ton feet,

$$M_0=M_3=0.$$

At any point in the first span

$$M=1{\cdot}6x^2-246{\cdot}06x.$$

At any point in the second span

$$M=14788-0{\cdot}95x^2-285x.$$

and the diagram for half the bridge is shown in $ODD'D''D'''$.

5. $OFF'F''F'''$ is the diagram for half the bridge when only the first span is covered with the rolling load.

6. $OGG'G''G'''$ is the diagram for half the bridge when only the first two spans are covered with the rolling load.

As the depth is supposed to be constant, the moment of inertia of each section is now supposed to be nearly proportional to the greatest bending moment which is ever found at that section during any of the above distributions of load, and the ordinates of the diagram AEE, &c., show the values which have been assumed for the moment of inertia of each section, these values being either 1, 2, 3, 4, 5, 6, or 7. If the girders had not been of constant depth, so that the moment of inertia might have been changed by altering the height of the girder, as well as the areas of the booms, we should have proceeded in a slightly different way to get the diagram of I. As it was, our students

found it advisable to use six thicknesses of plates in the booms at the middle of the centre span, and they therefore made I there equal to 6.

It is unnecessary to enter fully into the details of the complete drawings exhibited. On each curve was written the scale to which it was drawn. Employing the methods previously described, a few hours will be sufficient for making all the necessary calculations.

The results obtained for the new distribution of I were as follows:—

In 1st and 3rd span.

With rolling load	Without rolling load
$m_1=63999$ ton feet,	$m_1=24000$ ton feet,
$g_1=21{,}030{,}000$,	$g_1=8{,}462{,}223$,
$F_1=1{,}387{,}000{,}000$	$F_1=488{,}910{,}000$.

$d_1=1486$,
$n_1=185040$,
$f_1=107540$,
$q_1=9{,}191{,}200$.

In middle span.

With rolling load	Without rolling load
$m_1=175500$ ton feet,	$m_1=85500$ ton feet,
$g_1=122{,}817{,}000$,	$g_1=61{,}975{,}000$,
$F_1=7{,}672{,}800{,}000$,	$F_1=3{,}938{,}100{,}000$.

$d_1=1988$,
$n_1=299310$,
$f_1=310180$,
$q_1=24{,}048{,}500$.

Just in the same way the necessary summations may be made for the most general cases, the moment of inertia and the load varying in any way whatever from point to point in a span.

Presents, December 11, 1879.

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